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# THE ASCLEPIAD.

A Book of Original Research and Observation

IN THE

*SCIENCE, ART, AND LITERATURE OF MEDICINE,  
PREVENTIVE AND CURATIVE.*

1884.

BY

Benjamin Ward Richardson, M.D., F.R.S.

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TERAR DUM PROSIM.

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#### ERRATA.

Page 42, line 22, for October 17 read 18.

„ „ „ 27, „ „ „ „  
„ 44, „ 8, „ *eighteeth* read *eightieth*.  
„ 91, „ 13, „ *produce* „ *produced*.

To

THE HONOURABLE SIR WILLIAM ROBERT GROVE, F.R.S.,

ILLUSTRIOUS AUTHOR OF THE THEORY OF CORRELATION OF FORCE,

AND

ONE OF THE JUDGES OF THE HIGH COURT OF JUSTICE,

*THIS VOLUME,*

WITH SINCERE ADMIRATION AND REGARD.

*Is Inscribed.*



## PREFACE

TO THE FIRST VOLUME OF THE "ASCLEPIAD."

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IN this volume I bring together the four parts of the ASCLEPIAD for the year 1884.

The favour which the work has received in its serial form is the best incentive I could have for a continuance of my labours.

BENJAMIN WARD RICHARDSON.

25, MANCHESTER SQUARE, LONDON, W.

*October 31st, 1884.*





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## MORPHIA HABITUÉS AND THEIR TREATMENT.

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**I**N my early career I once met one of the most remarkable representatives of opium eaters who ever lived, and from the late Professor Easton, of Glasgow, his medical attendant, I heard the clinical history of that extraordinary man. Later in my life, namely, in 1869, I demonstrated before the Medical Society of London the negative action of opium on birds, in illustration of the then novel discoveries of Dr. Weir Mitchell, of Philadelphia.

The first of the facts here named fixed my attention on the physiological study of the action of opium. The last, in the most unexpected way, brought me into practical communication with the subject to a somewhat unusual degree. Within a week after the report of the experimental demonstration made before the Medical Society, I was waited on by a member of our own profession who had habituated himself to the use of opium, and who had become desirous of being relieved of the infliction. He was soon followed by another person who was under the same condition, and

[*Essay read before the Medical Society of London on Monday, December 10th, 1883.*]

from that day to the present I have rarely, if ever, been without a patient similarly affected.

The appearance of such patients in the consulting room is by no means regular. Sometimes I do not see one for many months; then, in one month, I see three or four. In the week ending November 24th, three such cases appeared; all of them new cases. I have noticed that when a series of new cases begin to appear, some old ones usually present themselves, and I name these facts because they seem to indicate either that there are certain times or seasons when the symptoms produced are more urgent, or when the desire to be freed from the cause of the symptoms is stronger than at other seasons.

I notice also in these general and introductory notes that amongst the habitués of whom I speak there are many who are given to consult by letter, and who decline to be seen personally. Certain of these act on this plan because they are anxious not to be recognized as devotees to the practice; but others follow the same course because they are too nervous to make a personal appearance, or to hear the opinion which they think may be expressed in regard to the condition in which they are and the mode of life they are to follow for cure. This proceeding is strikingly characteristic of an advanced stage of induced disease, when the nervous system is very severely affected. I have consequently learned to look upon persons who firmly decline to be seen as deserving of special attention and care.

There is another class of habitués which has a distinct peculiarity. The members of it are habitual devotees, but, according to their view, they do not suffer permanent injury. They are not anxious about

themselves, but they are communicative. They seem as if they were impelled to tell some one what they are doing, and what they experience, and why they fell into the habit. Some also of the opium habitués are exceedingly intelligent; and I shall have occasion, as I progress, to read a report made by one of them, bearing upon many points connected with the practical subject in hand.

There are three modes by which the opium habitués indulge :—

- (a) By swallowing opium in the solid form or in solution.
- (b) By smoking opium and inhaling the fumes.
- (c) By the subcutaneous injection of morphia.

In the beginning of my practical life in medicine, the only mode of opium-taking in this country was by the first of these processes. Such a person as an opium smoker was scarcely known to exist in English society, while the injection of morphia by the subcutaneous method was absolutely unknown.

Of late years these positions have been changed. Opium eating or drinking has become less common; opium smoking has gained some footing; and, since the discovery of the process of subcutaneous injection, dating from about the year 1859, the practice of subcutaneous injection of morphia has increased steadily, until it has now become a formidable enemy to health. Owing to the extent of the subcutaneous method, owing, also, to the fact that I have a wider knowledge of it than of the habits of eating or smoking opium, I shall in this essay confine what I have to say to subcutaneous morphia administration. I will consider the subject under six heads :—

1. The circumstances which commonly lead to the habit.
2. The time required to render the habit confirmed.
3. The degree to which the poisoning may be carried.
4. The phenomena produced.
5. The dangers incurred and the ultimate results.
6. The mode of cure of the habit.

#### THE BEGINNINGS OF THE PRACTICE.

In the larger number of instances of the habit of subcutaneous injection of morphia, the reason assigned for the beginning of the practice is alleviation of pain. There are exceptions to this rule, certain of which are very singular. One person assured me that he began in order simply to ascertain what the effect was like, and that finding it not unpleasant, he went on until he felt as if he could not live without its continuance. Another explained that he resorted to it from imitation, that a friend who lived with him had recourse to it, and that "he followed suit," but could give no definite reason why. Three have been led to the practice through alcoholic excess, under the idea that they might by this method free themselves from the craving for alcohol. Two or three have assigned sleeplessness as the first motive, but much fewer than might have been expected name this special provocative. One, a very distressing example, named "sleeplessness" with "mental torture"—I use the precise terms employed—as the primary causes. A few have stated that the habit was acquired by first having had resort to it for the treatment of disease that was not accompanied by pain, but was thought to be fatal. One of these was a sufferer from diabetes, two from persistent intermittency



of the heart. But, as I have already intimated, the alleviation of physical pain was the excuse, valid or invalid, which, in the large majority of instances, was adduced as the starting-point of the practice, the pain commonly referred to being neuralgiac in character. Sciatica is the form of neuralgia which I have heard most frequently named. The following is a verbatim account from one habitué who has voluntarily sent me a complete history of his case :—

“For some years I suffered misery from a morbidly scrupulous conscience, a weak will, and a dyspeptic diathesis. But at thirty I began to educate myself and take pupils, marrying soon afterwards. Ten years later I was completely prostrated by sciatica, and for the next decade could only do light work,—honorary,—pursuing, however, my studies with what strength I had. This brings me to an age of nearly fifty. Just then my wife died of cancer. For some months I had been accustomed to give her relief, always with the doctor’s direction, by administering morphia hypodermically; and when another attack of sciatica threatened me, I applied the same remedy to myself, beginning with one-sixth of a grain once in the day. The effect of the injection was not only to stave off sciatica, but to give me both pleasure and power. Previously to commencing the habit I was *very dyspeptic*, my tongue always moist and loaded. I was not able to study until some hours had elapsed after a meal. I was limited to the same extent after walking. I was much troubled with various conscientious scruples, and often, I fear, vexed my neighbours by my irritable temper. I frequently took a bottle of claret a day and a glass or two of grog at night to make life tolerable and sleep practicable. Sleep was such a difficult thing to me

that I might have said, 'Others sleep that they may live, I live that I may sleep.'"

He then goes on to say that from commencing with the small dose named above, one-sixth of a grain, he largely increased it, and at last came to an average of three-and-a-half grains per day, at which quantity he steadily sustained the habit for four years.

I shall refer to this gentleman again, in order to show that he defends the practice on the ground that he is happier and better for it. But I adduce him at this moment because he has given, in what I have read, the most common of all histories of the origin of the habit, and because he is a representative type of the class of person who is most susceptible of falling into the habit. It will be seen that he is a man of nervous temperament, with bilious admixture of temperament; thoughtful, anxious, excessively, I must add morbidly, conscientious. He is naturally touched acutely by what other persons less sensitive would consider moderate and endurable pain, and having once tasted relief derived from the narcotic injection, he continues it until the habit becomes, in fact, a second nature. His nervous centres are never free from the presence of an alkaloid, which forms what may be almost considered a new physiological combination with them.

If I adduced any number of further illustrations I should not be doing much more than repeating the one I have cited. His record is peculiar only in that his habit was purely voluntary. He had no advice from any medical man on the subject; the most that can be said on the matter being that he had learned how to perform the subcutaneous operation from the circumstance that his wife, under most justifiable and, no doubt, necessary conditions, had been subjected to it.



One feature of this case, however, demands attention, because it is a feature which belongs particularly to many other cases. It will be observed that the writer speaks of having been habituated to the use of alcohol before he commenced to inject himself with morphia. He was not what would commonly be called an intemperate man, but he had learned to rely on alcohol to a considerable extent. He often, he says, had to take a bottle of claret a day, and a glass or two of grog at night, to make life tolerable and sleep practicable. He suffered, as a result, from constant alcoholic dyspepsia. The state is one which very often proves the starting-point of the morphia practice. It is as if the person moved from one habit to another, and as if the first induced the second. It does not follow that the morphia practice puts out the alcoholic,—indeed, that rarely occurs,—but no doubt the resort to alcohol is modified by the action of the morphia.

Amongst total abstainers from alcohol the morphia habit, if not unknown altogether, is unknown to me. I have never met with an example, and my knowledge of the abstaining community is not inferior to that of any physician who has the means of inquiry at his command. This fact influences me much in concluding that alcohol, acting like an additional worry and wearer of life, is an important secondary agent in the production of the opium habitué and of subcutaneous injection of morphia as an habitual practice.

#### TIME REQUIRED FOR FIXING THE HABIT.

Opinions need not vary very much on the question, How long a time is required before the practice of injecting morphia becomes a fixed habit demanding particular treatment for its cure,—treatment, that is

to say, independent of the mere determination of the patient? In other words, how long is it before the will of the patient is rendered altogether subservient to the effects which the morphia induces?

I notice that Dr. Mattison of New York, who has sent me several essays on the subject now in hand, considers that in a month the habit is completely established. We may readily accept this term, and I think, on the whole, it is correct. I believe the craving is usually established earlier than in a month, if the injection be pursued regularly once or twice in each twenty-four hours, and if it be found pleasant to increase the dose of the narcotic steadily, as is too often the fact.

But while we may consider that the desire for repetition of the morphia is fixed by a month's indulgence, it must not be inferred that the same serious difficulties are then experienced in breaking away from the practice as are encountered when it has been carried out for a longer time. I have had, I confess, great difficulties in persuading the habitué of a month or six weeks to cease, and I have failed altogether with one who was, I believe *bona fide*, of this class. They are, however, more amenable to treatment. Much depends in every instance on the tendency of the affected person for alcohol. If the alcoholic tendency be great, the narcotic craving is sure to be intensified. In a word, the first habit often quickens while it lessens the second.

I have more than once met with a case where some other narcotic practice has preceded the morphia intoxication. I had for some time under observation a gentleman who combined chloral drinking with morphia injection. An inability to sleep and an intolerant

aching in the temples, with tension and pains in the eyeballs, was assigned as the first provocative to the chloral. In fact, an excess of alcohol, though it was not freely admitted, was the first cause of all the mischief. That led to a persistent alcoholic dyspepsia, to worry of mind, irritability and restlessness. To obtain sleep, chloral was sought as a remedy. Chloral produced a heavy sleep, but it did not relieve the pain, and so morphia injection was the next step; and in a few weeks, morphia became the dominant narcotic. The chloral was now suspended, or was only resorted to at intervals, while the alcohol was, perhaps, taken less freely. The morphia was soon so definitely master that it became, apparently, as necessary as food, and was taken as if it were a meal.

It is impossible to imagine, unless it is witnessed, the hold which the morphia narcotic has upon its victims. One of the habitués, a medical friend of considerable eminence, who began the habit in order to relieve sciatica, who was perfectly aware of all the consequences of the habit, who was constantly coming to me to talk about them, who was never off the statement that he would give it up, would, without waiting in his conversation and protest, take the bottle and needle out of his pocket and charge himself with a dose in my presence.

A lady who was for a while under my care, and who was dotted all over those parts of her body that were within her reach with the marks of the punctures she had inflicted, would beg, while imploring me to get her out of the practice, to allow her to show me how she did it.

I have noticed in these very confirmed examples of the disease that the system becomes so sensitive, or

insensitive, to the narcotic, that the dose can, as it were, be weighed by the sense of its action. I have seen the attempt to cure by reducing the quantity of morphia in the solution utterly fail because of this sensitiveness. The intention has been detected at once from the effect not being complete, so that the difference of half a grain would not escape observation and adverse comment.

THE DEGREE TO WHICH THE TOXIC EFFECT MAY BE  
CARRIED.

The dose of morphia commonly resorted to in the first instance is very small compared with what is afterwards used. A sixth, or a quarter of a quarter of a grain at most, is first injected. Very soon, usually within three or four days, this dose is felt to be losing in power, and, as a rule, is steadily increased. I notice, also, as a general fact, that the dose once increased, is maintained, although it may be repeated in the twenty-four hours. I mean by this that if a dose of one-sixth of a grain be raised to a quarter of a grain, the quarter of a grain will be the next dose, however soon it may be taken afterwards. "We never retrench," was the observation of one of these habitués; and when they are left to themselves, this, I believe, is a very correct description.

The increase of the dose is ordinarily followed up in consequence of the fixed dose losing its effect; I use the term that is mostly offered, "losing its effect," although it is sometimes a difficult matter to define what is precisely meant by the statement. No two habitués explain in the same terms what effect it is that is lost. One will say that the dose, kept always at the same amount, fails to allay the pain, if pain be

complained of. Another will say that the fixed dose fails to give quiet. A third will affirm that the fixed dose begins to produce restlessness, and that it must therefore be increased in order that the remedy may not become worse than the disease. In time, as the new physiological constitution is developed, a variety of other statements begin to be named which are so incongruous and vapid that they admit of no reasonable interpretation. One patient, for instance, argued that it "was easier to inject a large than a small dose." Another, that "the larger dose caused less local irritation;" and so on, as if any excuse whatever were sufficient without the trouble of thinking over the subject for any better excuse. When the patient is in this frame of mind, he affords evidence that the practice has proceeded very far, the listlessness of mind becoming, as it were, an index of a perverted mental state which is never absent in the confirmed sufferer.

The dose is increased step by step, a fact which accounts for the enormous increase which may be attained without direct danger to life from instant toxic action. There have been a few cases in which a large dose has directly killed; but when such an event has occurred, it has been due, I believe, either to an accident in measuring the dose, or to an intention to commit suicide. I have known no example myself in which the dose has been suddenly raised to a dangerous point of activity. From a sixth of a grain to a quarter; from a quarter to half; from half to three-quarters; from three-quarters to a grain, in steady proportion, is the usual method of progress.

Few confirmed habitués stop short at anything less than three grains in twenty-four hours; some go on to five or six, and continue at that quantity for long



periods. If they do not exceed the first-named dose, they may continue to follow their usual occupations, and may not be discovered to be habitués. I know one person who takes three grains a day, and who is so ingenious in the administration, and so systematic, that I doubt if any one suspects him. He takes his dose of a grain on going to bed, the same dose on getting up, and one other similar dose some time in the course of the day, "as he feels to require it." He has learned, so runs his plea, "to know by experience when the morphia is required," though he can fix no precise time. He quotes De Quincey, that his desire is like the Roman centurion's soldiers, it always comes when he calls it, but sometimes it comes when he doesn't call it. If the dose taken exceed much over three grains a day, there is left very little power for very active work, physical or mental. Usually with doses exceeding three grains, there are soon some sensations of exhaustion and of mental incompetency, which tell their own story and destroy the regular course of life and duty.

At the same time it must be admitted that the quantity that can be tolerated without destruction of vital action, and tolerated, too, for long intervals of time, may greatly exceed three grains. The largest dose I have ever known of as being taken in the twenty-four hours was eighteen grains; but as this was an exceptional case, which did not come directly under my own observation, I do not vouch for the accurate measurement of dose. From ten to twelve grains is not out of the way in confirmed habitués, who have learned frequently to repeat the administration. Dr. Mattison has published the history of a case, written by the habitué himself, of a physician who took twelve grains a day for a considerable time, and in three years

had injected into himself six thousand grains, but now has broken himself from the habit altogether. The patient, part of whose self-written history I have read, has taken, I compute, four thousand grains, in smaller doses, extended over a rather longer time, and continues in what he considers to be good health. In another instance, which I knew, but which ended fatally, six grains a day were, I calculated, taken for two years or over, four thousand two hundred grains in the entire period. It was commonly taken in quantities of a grain and a half four times a day.

In summary on this head, I think it may be assumed safely that a dose of fifteen grains of morphia, divided into several administrations, may be taken daily without immediate toxic effects of a fatal or immediately critical kind, when the administration has been slowly carried up to that amount. I do not think that many morphia habitués exceed six to eight grains a day. The majority, I should say, limit themselves to from one to three grains.

It is of some moment to be conversant with the kind of solution employed by the people for the purpose of injection. I have been more than once told that there is a mixture sold from a prescription, the precise formula of which has not been published. I do not believe there is any truth in the statement. It may be that a few persons have used a prescription, written by some physician, who for a distinct and legitimate reason has written it. It may be, also, that some medical men, who have got into the habit of using a solution, have prescribed their own form, and by these means, one or other, the rumour may have floated.

As a matter of fact, I find, on inquiry, that the fluid for injection sold by the chemists is the solution of the Pharmacopeia, the dose for which is marked as from

one to six minims. The solution, as we know, is an acetate, and is the same as that which we ourselves ordinarily prescribe for hypodermic use. I have been informed by one first class dispensing house that sometimes Squire's aqueous solution of the bi-meconate is asked for injection. Beyond that I can discover no evidence of any particular form of solution being popular or in anything like public demand.

The habitués tell us, generally, that they commenced the habit either by being first submitted to the operation by a medical man, or by having seen it properly performed on some one else. They begin, in nine cases out of ten, by injecting five minims. They increase gradually, and they attain at last the largest increases by frequent repetition of as full a quantity as they can get in from one puncture of the needle. They are not commonly afraid of pain from the puncture; but to this an exception is now and then found. One of our brethren, who was an extraordinary habitué, was so sensitive to the needle that he resorted to ether spray in order to produce local insensibility. He froze a small surface from a minute ether jet, and punctured by the side of the frozen part where the skin was insensible but not hard. He was of opinion, that by the plan named he saved the mark which is usually left, as well as the pain; and he was enabled to inject an immense number of times near the same spot, as if he had induced a false absorbing sac or pouch which acted as a receiving cavity.

#### THE PHENOMENA PRODUCED.

When the injection of morphia is kept within reasonable limits, the phenomena which occur from it, subjective or objective, are often obscure.



The subjective symptoms, when they are marked, are sleeplessness, if the narcotic be not indulged in, and a perturbed sleep when it is; an impaired appetite or an appetite very capricious; a deficiency of mental capacity, especially of power to will; a drowsy melancholy, apt to pass into apathy or moroseness; and, occasionally, a semi-hysterical state, with tendency, particularly in women, to emotional excitement followed by depression. The physical powers are, I think, always reduced, and failure in muscular power for sustained efforts is perhaps one of the earliest serious symptoms. I should characterize the muscular action as slow rather than feeble. It is as if the muscles were not excited by the will to quick action. There is, however, true feebleness, which is quickly shown after a rather prolonged exercise. There is much dislike to change of life, great talking about the desirability of change, great expression of resolution for change, but no actual carrying out, voluntarily, of that which is so much declared for. The subjective phenomena of chilliness or coldness of surface are rarely, if ever, absent, although they are exchanged, frequently, for passing sensations of a feverish character without thirst.

Some peculiar sensorial nervous phenomena, in the way of deficient or perverted faculty, are often presented. The sense of smell may have become so modified that one odour is undistinguishable from another, or every odour may be resolved into a particular odour which has at some previous time made a special impression. Thus one person had a fixed odour of benzine whenever the olfactory sense was touched by any perfume or smelling substance.

Another experienced an odour like that which emanates from a waning wick or candle. Taste is usually affected much in the same way as smell. It is either perverted so that one substance can scarcely be distinguished from another, or the sense is more particularly susceptible to one special thing, or is deficient for everything.

I have not ascertained correctly whether touch is seriously affected, the means of determining the fact not being easily at command; but I have noticed that habitués are clumsy in the way in which they touch objects, and, as it were, undecided and hesitating. How far the senses of hearing and sight are influenced I have also not been able to discover with much exactness. Investigation is here required. Most habitués complain of being oppressed by shrill sounds, which may be due to irritability of mind, and some complain of an aura and of a painful oppression, as if the ear were blocked up. To all any prolonged exercise of the sight is tiring, and for that reason reading is very rarely followed for any length of time. The retina is often intolerant of bright light.

The objective phenomena presented by the morphia habitué are fairly characteristic. During all the time in which the habit is carried on the temperature of the body is irregular. Soon after the administration of the morphia there is a rise in the temperature called by some observers neurotic pyrexia. It has been known for a long time past that the action of opium upon the body is to raise the temperature, and pyrexia has, therefore, been accepted as one of the diagnostic signs of poisoning by opium. The sign is, I think, never absent. In the most determined habitué there is seen, soon after the dose is taken, a

pyrexia which lasts for a period of two to three hours. The temperature may rise to 104° Fah. I have rarely known such an increase, but 100° to 101° is by no means infrequent. The rise in temperature is attended at first with dryness of the mucous and cutaneous surfaces, ending in a secretion more or less excessive. Profuse excretion from the skin may sometimes occur, and free excretion from the bronchial surfaces and salivary glands with copious expectoration is a symptom by no means uncommon.

The circulation in the morphia habitué is slow, as a rule, and the pulse is small and resistant. The resistance is well marked when the patient is fully under the influence of the narcotic. As the effect goes off, the pulse relaxes and the action quickens, but the acceleration passes rapidly into feebleness, and then the motion grows slow again. The period of feebleness is the period of desire for a repetition of the narcotic.

The action of the heart is regular and unattended with any unnatural murmurs due to the morphia *per se*. The natural sounds, while the action of the morphia is in full force, are short and sharp. Later on the sounds are fuller. The motion of the heart is not made irregular by the morphia at the time, and, I believe, never intermittent. But as the effect of the narcotic tones down, there is distinct irregularity and sometimes a reduplicated second sound.

The respiratory sounds are rarely quite natural. At one stage the vesicular murmur is dry, with soft bronchial cooing murmurs; at another stage there is moist bronchial rale.

The tongue of the confirmed habitué is usually dry, and rather more red than natural. It is not often

loaded or white. The bowels in almost all cases are constipated, but what we call ordinary dyspeptic symptoms do not give much trouble. In this respect the toxic action of morphia is extremely different from that of alcohol. There is no flatulency, no distension, no colic, no pain in the alimentary canal. Dislike for food or carelessness for food with what is described as a depraved appetite are the dyspeptic phenomena that are chiefly noticeable.

In the commencement of the practice, nausea and vomiting are sometimes severe symptoms. They may continue for many weeks. They may recur at intervals even in the most confirmed habitué, and they may cause much distress. They rarely are sufficiently painful to check the habit. On the contrary, as they subside under the habit, they are feared as symptoms belonging to every attempt at abandonment of it; and not without cause, since, by a strange rule, they nearly always are presented when the attempt to break from the habit is carried out.

The action of the kidneys is irregular, but excessive or frequent excretion of urine is not marked. In nearly every case the urine is not voided until the bladder is largely charged. This symptom deserves special observation, for as the reflex action of the bladder becomes less decisive the bladder may be filled to dangerous distension without the patient being aware of the condition. The bladder may, under these circumstances, become temporarily paralyzed, and may require to be relieved by the catheter. The urine is scarcely ever charged with deposit. Even during the neurotic pyrexia it is not very high coloured unless there be some complication. The colour of the urine at nearly all times is pale, the reaction neutral.

The expression of countenance of the morphia habitué is of one older than his years. The expression is careworn. The sallowness which some observers have described I have not particularly recognized. The eye is dull; the pupil is either contracted or considerably dilated, according to the stage.

The moral condition of this habitué is very different from that of the confirmed alcoholic. The opium habitué is not *intentionally* untruthful. He makes confession rather than concealment of his habit. He is not violent; he is not humorous; he is not mischief-making. He may, under the additional influence of wine or spirit, be rendered for a short season hilarious and witty, but, apart from such influence, he is under a cloud of depression which never completely leaves him and is usually severe.

Keeping within the strictest bounds of description from nature, I have in the above passages sketched the condition of the morphia habitué who is in the stage during which recovery is fairly possible. That the account may carry with it the strictest impartiality, let me turn once more to my friend who has sent me the latest confessions of an intelligent representative of his class.

This gentleman, be it observed, makes no apology for what he is doing. On the contrary, he defends the practice. Comparing himself with what he was before he commenced the practice, he declares that, in regard to dyspepsia, power of study, power of memory, power of walking, temper, alcohol drinking, sleep, diet, he is a changed man for the better. None of his recent acquaintances would guess that at one time he had bad temper.

The practice, adopted with a full knowledge of its



dangers on the moral side, and, so far as they are known on the physical, is attended with all the above-named advantages in his case, as he believes.

The bad results of which he gives an honest account are summed up in the subjoined observations :—

- (a) Torpidity of the liver, counteracted by use of podophyllin.
- (b) Abscesses forming in the neighbourhood of the puncture, twelve of which have occurred, only one of them reaching to any depth, but most of them painful.
- (c) Tendency to thickening of the outer integument in points; a free secretion, attended on waking in the morning with copious expectoration from the bronchial surface; and an obstruction, at night, in breathing as if from one of the nostrils being choked up.
- (d) A difficulty in swallowing morsels of solid substance like a pill, as if the object were stopped in its passage by becoming enveloped in a layer of tenacious phlegm or mucus.
- (e) A tendency at times to violent perspiration, not habitual, but seeming to occur when the drug has almost deserted the body, or when the system has been much weakened, as by an abscess.
- (f) On attempting to break off the habit a sense of an unpleasant smell, like that of a candle which is just going out, and with a corresponding taste in the saliva, which fluid is, however, quite odourless to independent observers.
- (g) Occasionally a difficulty of articulation, such as is caused by taking an excess of alcoholic

liquor, and once, and once only, although the amount of morphia taken that day was less than usual, distinct signs of intoxication, of which he was quite conscious, but was helpless to suppress or to conceal.

- (h) Impotency, which is complete.
- (i) Difficulty of breaking off the habit of injecting the morphia; with diarrhoea and extreme and long-continued debility at every attempt to break it off.
- (k) Mental exaltation and depression, which would be great did he not arrange for the reaction to synchronise with sleep, so that it is only when he first wakes in the morning that he suffers from weakness and depression.
- (l) Much excitement in reading works of fiction, the imagination becoming so excited as to lead him to call out, though naturally he could read the same works with perfect coolness.
- (m) Morally a reduced sensitiveness and adaptability; an inclination to take things more apathetically than he otherwise would; and "less go" than he formerly felt, except during brief periods of exaltation.

In relation to exaltation of mind he has noticed, at times, a tendency to magnify and exaggerate dangers and their sources of feeling. He has detected himself in going through a railway tunnel or in waking up in the dark in a state of great, once or twice almost uncontrollable, alarm. In each case indigestion may have been a partial cause of the alarm.

These are the results of about four years' hypo-

dermic injection on a daily average of three-and-a-half grains, with no increase for two years past except under special circumstances, followed quickly by reduction to the usual amount of three-and-a-half grains in the twenty-four hours.

The history thus rendered supplies very faithfully the minor phenomena produced by the habit of morphia injection. I must now turn to the major phenomena incident to deeper indulgence.

The symptoms in the more determinate form are, subjectively, exaggerations of those which have already been described. The most prominent symptom is the determined appetite for the morphia and the inability to appease it. As this appetite intensifies, the will power, already reduced, is practically destroyed. There is no real care for anything in life, no true repose, no happiness. The ordinary signs of the toxic action of opium are not observable. There is no coma, no special drowsiness. No one particular mental faculty fails, but all faculties are reduced until at last there is general failure, enfeeblement of all the faculties together, with similar failure of the voluntary and involuntary muscular powers.

The objective symptoms are those of diminished vitality generally. The mind is seen to be uncertain and weak; the limbs are seen to be feeble, and the body is usually brought towards the condition in which it is known in old age. The pyrexia is less intense than in lighter stages, and is more evanescent. The digestive functions are impaired, but emaciation is not by any means a necessary condition. There are no specific local signs, none pointing to any one particular nervous lesion. The life declines altogether, and I should rather compare the final failure, which



I have now witnessed in three instances, to death from premature senile decay than to any other mode of dissolution.

There are many incidental points of a diagnostic character which might with advantage be introduced here did not the introduction bid fair to lead away from the subjects which are more immediately under review. But there is one to which I would for one moment refer, in parenthesis, and which relates to the morphia pyrexia as a diagnostic sign between the action of alcohol, chloral, and morphia. Under alcohol and chloral, the evanescent flush of surface heat is followed by reduction of temperature. Under morphia, the pyrexia is sustained, as so often happens after injury to the brain. In cases where peculiar symptoms of a narcotic kind are manifested, the observation of these different results is most useful in diagnosis. The late Mr. Peter Marshall, a former President of the Medical Society of London, once brought me a patient whose case was an extreme puzzle. The patient had repeated attacks of an intoxication of some kind, but what the producing agent might be was the difficulty. She kept her own counsel, and, in consequence of the strict watch that was sustained, it was confidently felt that alcohol was not the enemy. I took the temperature of this patient during a paroxysm, and found it to be  $103^{\circ}$ , which of itself excluded alcohol and chloral as the cause of the pyrexia; and, as Mr. Marshall afterwards detected that the same rise of animal temperature regularly accompanied the toxic paroxysm and remained for a long period, I diagnosed opium or morphia as the cause, which diagnosis turned out to be perfectly correct.

## RESULTS AND DANGERS INCIDENT TO THE HABIT.

The results and dangers of the habit of morphia injection are pourtrayed in the account of the phenomena. It is clear that the greatest danger is the confirmation of the practice by the habitué, and the ever-increasing desire for the poison. There are very few persons who, like my correspondent, have the power to stop at three grains and a half a day when they have once attained to that necessity of desire. Out of thirty such persons whom I have met with in fifteen years, and on the information derived from whom this paper is constructed, he is the single exception, and the trial with him is not yet over.

There are certain results which, if not actual dangers, are serious disadvantages. Setting some of these, which we may call minor, aside, and including even in the minor the pyrexia and occasional excessive perspiration, there is, in moderate examples of the habitué, the slavery of the habit, which alone is sufficiently baneful. There is the perversion of sensory function. There is the impotency which in the male subject seems to be the common result. There is the mental exaltation and depression. There is the apathy, the disinclination to follow any active pursuit. There is the easily-produced muscular exhaustion. In immoderate examples of the habit there are the positive dangers of apathy extending to moral paralysis, of failure of mental capacities, of premature decline.

As a set-off against these bad results in the common run of cases, it is strictly fair to name exceptions in which it may be said, with all truthfulness, that the habit has, in a tentative point of view, been good as a method of treatment. I knew an instance in which

a member of our profession, who was suffering from diabetes, found that he could control the excretion of urine, and live an artificial but passable and useful life by injecting into himself one to three grains of morphia in twenty-four hours. I was so sure of the soundness of this practice that I sustained him in it, and for a period of eleven years it went on systematically. The effect of the injection was so definite that the quantity of urine excreted could be determined by the dose of the remedy. Ultimately this patient died from what might justly be called premature old age, but he attained the sixty-sixth year. He was fairly happy up to within a few weeks of death; and I have every reason to believe that his comparatively easy life was prolonged, by the morphia, many times the period over which it would have extended had the diabetic affection been permitted to run its own course.

#### TREATMENT OF THE MORPHIA HABITUÉ.

There are two primary questions to bear in mind when the treatment of the morphia habitué is under consideration :—

- (1) Is it right or necessary to interfere at all?
- (2) If it be necessary to interfere and stop the practice, how is it best to conduct the interference?

Touching the first of these questions, I have learned now always to make the strictest preliminary inquiry into the motive that led to the commencement of the habit, and into the fact whether, if the original motive were reasonable, the cause for it remains.

If the motive were originally sound, and if the cause remain, I endeavour to judge whether the affection

which the morphia is used to control is of more importance than the habit. If I think it is, and if I do not see any better method of treating the prime evil than the method by morphia, then I only interfere to the extent of regulating the administration of the morphia itself.

If, for example, I find diabetic symptoms favourably controlled by the morphia, I do not interrupt, necessarily, the reasonable continuance of the administration. I merely suggest regulation. If I find a person suffering from malignant disease which is out of the range of surgical art, and if this patient is relieved of pain and finds life rendered endurable by the injection of morphia as a habit, I only interfere to the extent of systematically regulating the dose.

If I find a person suffering from extreme or permanent intermittency of the pulse, and that relief from such intermittency is secured by the injection of morphia and by nothing else,—an experience by no means rare,—I only interfere to the extent of systematically regulating the dose.

The practice in my hands changes, however, from this plan if pain alone, pain, I mean, apart from clear signs of organic disease, is presented. If the pain be said to be neuralgiac, the most frequent originator of the habit, “tic,” or “sciatica,” I endeavour to dissuade from the habit, and try to give relief by other means, should relief from pain still be required. In such trials it is often found that when the morphia is suspended there is no pain to combat; there is nothing left to combat, except the special desire, the feebleness, and the absence of power of will which have been induced by the remedy.

Touching the second point of practice, namely the

best means of breaking the habit, when it is felt to be necessary to break it, there are three matters which require consideration, and on which medical opinions are widely different.

Some endeavour to break through the habit by giving a substitute, too commonly alcohol or chloral, for the morphia. This practice cannot, I think, be accepted as sound. It is handing the sufferer from one enemy to another. I have never seen anything but evil from this course, and I have twice witnessed results from it so exceedingly bad that I cannot for a moment hesitate to condemn it out and out. I have said that the method hands over the sufferer from one enemy to another ; I have known an instance where it cast a man on all three at once, to the rapid destruction, first of his mind, and soon of his life by general paralysis.

Another line of curative practice for the morphia habitué is that which has been quaintly designated "the tapering off plan." This consists in the gradual reduction of the dose until none of the drug is required, coupled with other modes of treatment of a general kind, such as carefully regulated diet, change of scene, and administration of what are still designated as tonic remedies, amongst which Bark and Quinine hold the first place.

I shall withhold comment for a moment on this mode of treatment in order to bring forward the third as a preliminary to the line of practice I shall myself venture to support.

The third method is that of abrupt withdrawal of the morphia and the actual restraint of the habitué until he has ceased to feel the effects of the loss, whatever his sufferings, nay, torments, may be during the ordeal.



Each of these plans has its advocates, and each advocate seems to be equally assured as to the soundness of his doctrine.

To my mind, the sudden withdrawal method would present every possible advantage if one quality were attached to it, namely, its rational possibility. There are, perhaps, some cases in which it is possible, but they are rare. When the habit has not been carried out for more than a few weeks, and when the dose injected is not more than half a grain to a grain in the twenty-four hours, then, I think, we may make a successful effort by direct withdrawal. Beyond this the method is, in my opinion, impossible. With alcohol it is possible and safe suddenly to withdraw the agent. With chloral it is possible and safe. With morphia the difficulty is all but insuperable. With the morphia habitué the sudden withdrawal causes direst symptoms of disease of the truest objective type. It is not merely what he, the habitué, feels, it is what he is seen to suffer. He vomits; he is purged; he is subject to colicky pains; he secretes saliva profusely; he is at one time bathed in perspiration, at another time he is icy cold with a dry skin; he is restless to an extreme; he refuses food or rejects it if he swallows it; he passes little urine; he goes through long intervals without sleep, and his craving for the narcotic is constant, and I had almost said maniacal.

To such a person something must be administered, and if alcohol, chloral, cannabis indica, henbane, or other substance be given for the morphia they do not satisfy. The symptoms continue until they are relieved by the one thing,—a re-administration of morphia. I believe the symptoms have been allowed, under the mode of direct withdrawal, to progress up to collapse; but here

I speak out of my own knowledge, because I have on no occasion permitted them to progress towards that extreme condition nor near to it.

The mode of direct withdrawal is, in my view, I repeat, out of the range of safe possibility, except in the limited class of cases to which reference was made a few sentences above, and there is this further objection to it that, when it has been carried out, the patient, having no confidence in the extreme measure and no sympathy or trust on those who have carried it out, relapses so soon as he is free. The patient is, in fact, like a child who with no will of his own to conquer an evil passion treats as enemies, to be disobeyed and distrusted, all who, by sheer force, conquer its desire, and who can only be led by kind and firm reasoning into the right path.

There remains now for consideration the system of treatment which consists in the systematic, firm, and gradual withdrawal of the morphia.

The plan thus named requires, in its turn, to be carried out with strict regard to rule. It is quite useless if it be extended over too long a period; it is also quite useless if it be supplemented by substitutes. I notice that Dr. Mattison allows seven days for the cure by this method. If he means that period as an average time I am with him; if he means it as applicable to every case that comes for treatment, I differ from him, though not largely. I should differ in lengthening the time according to the dose that has become habitual. I mean that if the dose is large and has been long continued, I do not consider a moderately longer time, say fourteen days, as too long absolutely. It is, without doubt, good to be as rapid as is safe, and if fourteen, or at most twenty-one days



are expended in the effort to produce a radical cure, all the time that should be given has been given.

In commencing the abstinence, is it best to reduce the number of administrations in the twenty-four hours, or to retain the same number of administrations and to reduce the dose at each administration? I have tested both methods, and I conclude that it is most practical to continue, at least for some days, the old times of administration and to reduce the dose each time. The dose must be reduced gradually, and no tricks must be played of doing it by injecting some other thing, such as water or tincture, without the morphia. That proceeding destroys confidence. If the quantity were six grains in the twenty-four hours, taken in one-grain doses, I should reduce it by giving five grains, divided into six equal doses, on the first day; four grains, similarly divided, on the second day; three grains on the third and on the fourth days; two grains, again divided into six parts, on the fifth day; one grain on the sixth day, and in like manner as to number of administrations. On the seventh day I should divide half a grain into six doses as before, but should drop one administration; and then day by day continue to drop one administration until there were none. These proceedings would occupy a period of about thirteen days, and if they by themselves, unaided by any surreptitious alliances with "substitutes," do not succeed, I fear that in the large majority of cases, nothing will.

Reviewing, indeed, all that I have seen, I am forced reluctantly to admit that success is quite the exception. At the same time I can consistently and faithfully feel that I have never laboured in any case without doing some good. I have checked a patient

from resorting to an increased dose ; I have succeeded in getting a dose reduced from a dangerous to a comparatively safe quantity. I have one habitué who from six grains per day has come down to one grain, and who writes to me from time to time reporting that the one grain suffices. I have another who has given up the habit altogether for nearly twelve months, and who, I have every reason to hope, will remain firm, though the temptation to relapse is often hard with him. On the whole, I have fair reason to be satisfied with the results.

What is now required for successfully combating this habit is united action, united knowledge, united will on the part of all practitioners of the healing art. It is in the endeavour to obtain this desirable object that I have brought the subject before the old and important Medical Society of London with which I have been associated all through my professional life, and from which I have received so many and distinguished favours.

## THE BEAD INHALER :

A SIMPLIFIED INHALER FOR THE ADMINISTRATION OF  
VOLATILE LIQUIDS.

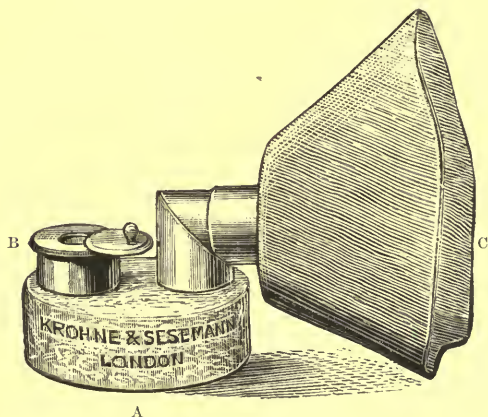
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**I**N the *Lancet* of June 9th of this year, I published a short essay on the administration of ammoniated chloroform for the reduction of zymotic pyrexia. In following up this simple and effective mode of treatment, I had to construct a new inhaler, which should possess the following advantages :—

- (1) Smallness and easy portability.
- (2) Steadiness for standing on the table at the bedside.
- (3) Readiness for receiving the volatile fluid and retaining it when not in use.
- (4) Possession of a wide surface for evaporation.
- (5) Easy adaptability to the mouth of the patient, by the patient herself or himself, while in bed, and in any position.
- (6) Cleanliness of action, so that different vapours may be used at different times without much trouble.

In the inhaler depicted on next page these practical wants are supplied. The apparatus consists of a receiving bottle, shaped like an inkstand (A) ; of a neck with regulating cap, through which the liquid is poured

into the receiver (B); of a mouthpiece (c), fitted inside with a talc inspiratory valve, and with an expiratory valve.



In the receiving bottle there is placed a string of glass beads, held on a line of cotton wick, or floss silk. The beads fill the receiver, and form a free surface for the volatile fluid, without danger of the escape of the fluid except in the form of vapour during the inhalation. From this construction, and in order to distinguish the instrument, I have called it *the bead inhaler*.

Inhalation from the bead inhaler is, practically, as free and easy as is the inhalation of common air, when the adjustment is complete.

From twenty minims to one or two fluid drams of the required volatile fluid may be poured upon the beads, according to the quantity that is necessary. When the cap is closed, the instrument is charged and ready.

In commencing to administer from the inhaler, the cap (b) is turned aside, much or little according to requirement. The cap is again closed when the inhalation ceases.

In changing one liquid for another, nothing has to be done but to draw out the beads, to wash them, if necessary, in a little spirit, and, after washing the bottle, to replace them in the bottle.

The inhaler can be used for ammoniated chloroform ; for the alcoholic solution of ammonia ; for alcoholic or ethereal-alcoholic solution of hydrogen peroxide ; for iodized ether ; for chloroform ; for methylene bichloride, and for any similar volatile medicinal substance.

The inhaler is also very well adapted for patients who are suffering from asthma or angina pectoris, and who find rapid relief from the inhalation of nitrite of amyl. Ten minims of the nitrite poured upon the beads yields a charge which, by taking care that the cap closes securely and the talc valve acts correctly, will answer, without losing effect, for twelve hours. Ether and the lighter fluids are not retained so long in the inhaler. They should therefore be used in the quantity required at each separate inhalation.

## *A PRACTICAL CLINICAL NOTE-BOOK.*

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**F**OR many years past I have kept a note or record book for morning practice; and as the book answers well, I venture to supply a copy of it in these pages.

The reader will find over leaf the record as filled up with one typical illustration. The number of the case marks also the pages of the volume, which is made up of several of these forms. I have five hundred, bound together. He will observe that the order in which the observation is taken follows in natural sequence:—1. The general facts. 2. The subjective phenomena. 3. The objective phenomena. 4. The diagnosis. 5. The prognosis. 6. The general and the medicinal treatment.

As a rule, in transcribing the subjective symptoms, I take down as briefly as possible the patient's own words. I find that in after times the patient remembers this better than words arranged for him, and that greater accuracy is in this way secured. To mark what is said by the patient I put the words in inverted commas, and so separate them from my own observation or comment. For sake of brevity, I also use contractions in naming diseases, as Ph. for Phthisis, Rh. F. for Rheumatic Fever. If the patient is losing flesh, I erase the word gaining, or the converse.



NAME.  
S. G.OCCUPATION.  
Solicitor.ADDRESS  
Lincoln's Inn.

EYES, blue.

HAIR, auburn.

TEMPERAMENT.—Nervous—Sanguine.

HEREDITARY TENDENCIES.—Father, living, 56, Rheumatismal; Mother died at 23 of

HABITS AND MODES OF LIFE.—Active life, indoor, (office) chiefly. "Temperate";

PREVIOUS DISEASES.—"Colds often"; Rh. F. when 14 yrs. old.

## SUBJECTIVE SYMPTOMS.

PAIN.—"Occasional short sharp pains in chest"; "stitch."

APPETITE.—"Not very good; often feels nausea after food."

SLEEP.—"Very fair, but sometimes wakes with dry cough."

MENTAL POWER.—"Not able to work or study as formerly."

PHYSICAL POWER.—"Very easily tired; gets drowsy at work."

OTHER SYMPTOMS.—"A good deal of flatulency attended with sense of depression; often feverish and thirsty; mouth dry on waking in the morning; face flushes under slight excitement; soon out of breath; frequent dry cough. Feels nausea after food, unless the meal be very light, and even then sometimes. Very easily chilled."

## OBJECTIVE SYMPTOMS.

TEMPERATURE.—100° F.

CIRCULATORY.—Pulse, 86; small, sharp; | *Heart*.—Action quick, feeble 90; faint  
sphygmophone gives third sound | anæmic murmur with second sound.  
accented.RESPIRATORY.—*Respiration* 20. Vesicular murmur clearer and louder on right than left side: wavy respiration under left clavicle, and slight dulness on percussion there; left sub-clavian murmur.

DIGESTION.—Tongue white, moist.

HEPATIC.—Function often inactive.

ALVINE.—Action slow generally: lately, occasional diarrhœa with pain.

RENAL.—URINE. 24 hrs. *Quantity*. *Colour*. *Reaction*. *Sp. Gr.*  
"Natural." Fale or high. Acid. 1.022.

CUTANEOUS.—At this moment dry; "easily perspires; occasional night perspirations."

NERVOUS.—Frequent nervous starts, especially on going to sleep.

SENSORY.—*Musæ volitantes*, now and then.

OTHER SYMPTOMS. Facial Expression two or three years older than natural, excited and wearied. Erythema fujax. Movements of body indicate general feebleness. Mind perturbed but hopeful. Had a paroxysm of short dry cough during visit. No hæmoptysis up to this time.

## DISEASE.

{ DIAGNOSIS.—Incipient Phthisis. Left apex affected.

{ PROGNOSIS.—Not favourable.



|                              |           |                  |                                |                              |                 |
|------------------------------|-----------|------------------|--------------------------------|------------------------------|-----------------|
| AGE.                         | RACE.     | HEIGHT.          | WEIGHT                         | LOSING.                      | DATE.           |
| 24.                          | Teutonic. | 5 ft. 7 in.      | 9 st. 10 lb.                   | <del>GAINING</del>           | Dec. 6th, 1883. |
| COMPLEXION, fair (freckled). |           | NAILS, delicate. | FINGERS { slightly<br>clubbed. | TEETH { small,<br>irregular. |                 |

Phthisis; 1 sister died of Ph. at 20; 1 brother, 23, alive and well.

"Smokes rather heavily."

DURATION OF PRESENT ILLNESS.—Two Months.

BATHS.—Warm, 90° Fahr., three times weekly.

CLOTHING.—Loose, warm, porous (Shetland under clothing), as directed.

EXERCISE.—4 hrs., at least, daily out-door, weather permitting, as directed.

CHANGE OF AIR.—Mentone recommended at once, if possible.

DIET.—4 light meals a day at regular hours, as per diet card. Smoking to be stopped.  
Half-an-hour to an hour's rest after meals.

#### MEDICINES AND OTHER CURATIVE MEASURES.

Syrup of bromide of iron, quinine, and strychnia  $\mathfrak{z}\mathfrak{i}\mathfrak{j}$ .

Of which take—

One fluid drachm in a wineglassful of water, three times daily, an hour after food.

Take also one dessert spoonful of cod liver oil with medicine; and continue same if it does not cause eructation nor produce nausea.

*December 20th, 1883.*

Patient returns to-day to report himself in every way better. Takes medicine and oil without difficulty, and believes with benefit. Is anxious to go to Ventnor instead of Mentone. Agreed. Ventnor for one month and then further report. Treatment to continue the same. Cough much less troublesome. Weight the same.

It will be seen that in this record all the principal facts are so set down that the whole could be taken in at a glance by another physician who had not seen the patient. In transferring a patient to another practitioner, I often send, therefore, a copy of the record taken on a loose sheet kept for the purpose. At the end of each of my volumes I retain one hundred plain leaves for an appendix, in which I carry on a further report of the case when the second page of the record is filled up.

I have now over a thousand reports of cases drawn out in this form. Apart from their immediate use, they are of value as affording many subjects of clinical study. I am, for instance, drawing out, for a future paper in the *Asclepiad*, an analysis of temperaments in relation to disease, derived from a thousand examples; and, as the reader will see, there are many other deducible lessons. The time required for completing each record, at first visit, averages about thirty minutes.

## HARVEY AFTER DEATH.

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**T**HE re-interment of the remains of the illustrious William Harvey, the final discoverer of the circulation of the blood, on the 17th of October last, was an event in the medical history of this country.

On the 3rd day of June, 1657, about ten in the morning, Harvey, then in his eightieth year, found, on attempting to speak, that he had lost the power of utterance; that, in the language of the vulgar, he had the "dead palsy" in the tongue. He had not, however, lost his mental faculties. He made signs to Sambroke, his apothecary, to bleed him from a vein under the tongue. Knowing that his end was near, he sent for his nephews, to each of whom he gave some token of his remembrance, his watch to one, his signet ring to another. On the evening on which he was smitten he died, the palsy giving him an easy passport. He went down with the sun. The precise place is not known: it was probably in the city of London.

From June 3rd to June 26th, a period of twenty-three days, was the time that elapsed between the death and the first burial of Harvey in the vault of the

Harvey Chapel, then recently built by Eliab, the brother of our anatomist, at the church at Hempstead, in Essex. Hempstead is an ancient village, about seven miles south-east of Saffron Walden, near a river called the Pant. There are in the parish about 170 houses. Near by was Winchlow Hall, a long time the seat of the Harveys after William Harvey's death. Hempstead church was, until two years ago, a beautiful structure. On Saturday, January 28th, 1882, the splendid tower fell, and the place is still in ruins, but the Harvey chapel remains entire.

There was for a long period of time a famous oak tree in the parish of Hempstead. It was known as the Hempstead oak, and was of enormous size. I remember it in its last days. It is now gone, under process of gradual decay. In a more savage day than ours, bull baiting was the grand sport in Hempstead, and a famous "ring" was frequented there. The ring remains in good preservation, though the baiting has ceased. One fine Sunday afternoon in 1882, driving to Hempstead with a party of antiquarians, I found the "ring" occupied by a body of Primitive Methodists, who had turned it into a temporary chapel. While my comrades were in the churchyard a little way above, I stole back to look at this assembly, so strangely placed. The scene was extremely picturesque. The trees which fenced the ring formed by their boughs the roof of the extemporized sanctuary. The preacher, I think, had been a woman, who in the centre of the group stood bare-headed, with a scarlet tippet over her shoulders, closing the service with the hymn beginning,

"Before Jehovah's awful Throne."

They were singing the last verse, and she, using her

hymn-book as a baton, was leading with much energy the words:—

“Wide as the world is Thy command,  
Vast as eternity Thy love ;  
Firm as a rock Thy truth shall stand  
When rolling years shall cease to move.”

They all sang in good tune, doubling the word “eternity” with tremendous emphasis ; and I thought they occupied right well the ground where the savages once raved at the dogs and baited the bull.

I beg pardon for a digression. I was observing upon the chapel which Eliab Harvey constructed in connection with the church at Hempstead, and the vault beneath the chapel in which the remains of William Harvey were first laid.

In the twenty-three days between the death and the first burial, many things had to be done. The case or shell had to be constructed. The body had probably to be embalmed. The body had to be carried from London to Hempstead. And one other thing had to be done of which I shall speak more particularly a little further on.

The case in which the body was laid is best described in the words of our learned Harveian Librarian at the College of Physicians, Dr. Munk, who defines it as a “mummy chest,” made of lead. It was a rough outline of the body. It was six feet three inches long. It was one foot nine inches broad across the shoulders, one foot four inches across the pelvis, seven inches at the feet. It was seven inches deep on an average, being deepest at the breastplate, *viz.*, ten inches there. On the upper surface of the chest was a breastplate measuring twelve inches by eight. On the breastplate was inscribed in quaint capital letters:—

DOCTOR  
 WILLIAM ◇ HARVEY  
 DECESED ◇ THE ◇ 3 ◇  
 OF ◇ JVNE ◇ 1657  
 AGED ◇ 79 ◇ YEARS.

The case was very much larger than the body of Harvey, who was a small man, not much over five feet, and of slender build.

The body was borne out of London towards Essex, followed by the President, Dr. Alston, and many Fellows of the College of Physicians in their robes and gowns; how many went and how far they went beyond the city walls we do not know. The *cortège* would probably go first to Epping, then to Bishop's Stortford, and next to Thaxted—at that day a considerable place—or to Saffron Walden, and so on to Hempstead.

The antiquary and historian John Aubrey was at the funeral, and helped to carry the body into the vault. Aubrey says, "He, (Harvey,) was lapt in lead, and on his body in great letters his name, Dr. William Harvey."

In the vault the remains so left remained from June 26th, 1657 to Oct. 17th, 1883. They underwent at last great risk of perishing. In 1847, I found the case or chest containing the remains lying among a number of chests, with coffins, containing other members of the Harvey family. I often repeated my visits during thirty-five years, and two years ago, at my instance, the Royal College of Physicians undertook the task of preserving the remains from further chance of decay, as the result of exposure to wet and air. The work undertaken by the College was completed on the day of re-interment above-named, Oct. 17th, 1883, when eight of us, Fellows of the College, carried up the repaired leaden case containing the remains



out of the vault, and placed it in a sarcophagus of Sicilian marble in the Harvey Chapel. Then the President of the College, Sir William Jenner, having laid the works of Harvey and a scroll with other records upon the leaden case, the sarcophagus was closed.

I have a longer history of all the details connected with this event, but that may rest in order that I may at this moment enlarge on the topic which forms the heading of this study, Harvey after death.

In the church at Hempstead, on the northern wall close to the Harvey Chapel, there is a bust of Harvey, which was put up soon after his death. It is a marble bust. The head is surmounted with thick, clustering locks, the face is well chiselled. One day, when I was in the church, I began to speculate at what period in the life of Harvey the bust was taken. In all the pictures I had seen of him he was represented as having very little hair on his head, and this luxuriance puzzled me. At last, by a sudden inspection, I saw what had happened. The face appeared to me as the face of an aged man in death. I felt convinced that after death, some rude sculptor had worked from a cast of the dead face, and had added the rest out of his own ideas. I was the more assured of this the more I looked. To be completely satisfied, however, on returning home I asked our English genius of Sculpture, Thomas Woolner, R.A., to go with me to look at the bust. He did so most kindly and promptly, and confirmed my view. "With the cast of the face for his copy," says Mr. Woolner, "the sculptor has shown true artistic delineation; but all that he has been obliged to add to make up the bust as it stands is of the worst possible quality."

To separate the cast of the face from the artificial



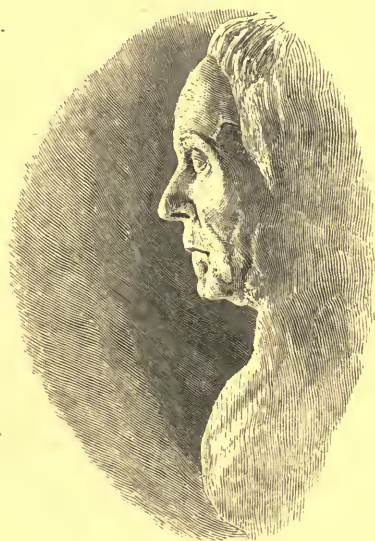
additions, I had a photograph taken in such a way that the face alone was seen in profile. This view was transferred to wood and was published in the *Lancet* of Nov. 30th, 1878, in my essay there on the remains of Harvey. By the kind permission of Dr. Wakley, I am enabled to reproduce the plate here.

In this delineation the reader will find himself face to face with Harvey, as Harvey, in his eightieth year, lay dead. In the Hunterian Museum at Glasgow—William Hunter's—there is an oil painting of Harvey, taken by a painter named William van Bommel.

As that painter was born in 1630, and Harvey died in 1657, the "picture must have been taken from a sitting" when the sitter was near the time of his death. The picture tallies with the likeness from the bust nearer than any other painting we have of our immortal discoverer.

A quiet, placid, beautiful face, even in death, this of Harvey. The dark eyes which Aubrey comments on flash no more. The expression, quick even to fierceness in young life, carries with it here that melancholic repose which Dr. Ent not many years before had observed, when after inquiry of Harvey if it were all well with him, he had received for answer, "How can it whilst the Commonwealth is full of distractions, and I myself am still in the open sea. And truly did I not find solace in my studies, and a balm for my spirit in the memory of my observations of former years, I should feel little desire for longer life. But so it has been that this life of obscurity, this vacation from public business, which causes tedium and disgust to many, has proved a sovereign remedy to me."

Now on the face we look at sits the eternal rest, when Commonwealths no longer distract, and when there is no more sea.



WILLIAM HARVEY AFTER DEATH, AGED 79 YEARS.

*To face page 44.*



*DISCOVERY OF A METHOD OF AN ANATOMICAL INJECTION OF THE BLOOD-VESSELS THROUGH THE CEREBRO-SPINAL CAVITY.*

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**W**HEN, between the years 1866-72, I was conducting my researches on the best modes of restoring animation after sudden dissolution from chloroform and other lethal substances, it came into my mind to ascertain if direct stimulation of the brain with oxygen or other gas would excite either the respiratory muscles or the heart into renewed action. I made, therefore, an inquiry on this matter.

It often happens in original work, that in carrying out a design which has been most carefully projected the original intention is not attained, but that some result which was never thought of is attained. From this circumstance experiment becomes, to him who will keep his eyes open to all that occurs, a safe passage to discovery, though the discovery be not that which was hoped for and expected.

In the present paper I am about to narrate one of these aside readings, if I may so name them, and which is very curious and instructive. What I expected from my research I did not get, and what I did get I did not expect.

The general outline of the experiments ran in this way. I allowed a warm-blooded animal, usually a rabbit, to sleep to death in the vapour of chloroform or other narcotic vapour. When death had taken place, when breathing had stopped and the heart-beat was no longer distinguishable by the stethoscope, I introduced a long subcutaneous injection needle into the cavity of the cranium from behind the eye-ball, through the optic foramen. The trunk end of the needle was connected with a piece of India-rubber tubing, and the tubing with a reservoir containing the oxygen or other gas. By means of a small bellows syringe, a portion of the gas was very gradually injected into the cranial cavity, and observation was directed to the muscles to ascertain if there was any responsive contraction. Practically there was no response on which any special stress need be laid.

In the first experiment, however, in which a large rabbit was the subject, when I proceeded, some time after the injection had ceased, to open the body of the animal in order to see if there was any irritability left in the heart or in the intestines, this very singular fact was disclosed. The iliac veins and the vena cava inferior were charged with the most brilliant red or arterial-like blood. The blood in the heart on both sides and the blood in the lungs was dark as compared with the blood in the veins specified.

Having noted the fact, I thought little more about it; but in another experiment, in which common air was injected into the cerebro-spinal cavity, and in a third experiment, in which carbonic oxide was injected, the same phenomenon recurred. I now began to consider and investigate the phenomenon as a distinct study.

## OBSERVATIONS IN 1869.

*Observation 1.*

A rabbit was allowed to sleep to death under chloroform vapour. So soon as the animal was dead, the abdomen was laid open, and the iliac veins and inferior cava exposed to view. The veins were filled with dark blood. The hollow needle was then passed into the cranial cavity by the optic foramen, and a jet of oxygen gas was transfused. Almost instantly the blood in the vena cava inferior became of bright red arterial colour, the colour gradually extending along the vessel, until, in time, it reached the right side of the heart. Some blood in the left side of the heart was much less red in colour than that in the inferior cava.

*Observation 2.*

An experiment precisely similar to the above was next performed, with the only difference that common air was injected instead of pure oxygen. The results were the same.

*Observation 3.*

An experiment was again performed, with the difference that carbonic oxide, which has the property of giving a bright-red colour to blood, was injected instead of oxygen or common air. The results were the same.

*Observation 4.*

I had observed that the venous blood in the sinuses of the brain and in the vena cava superior was not changed in colour by the injection; but in order to determine whether the change of colour would occur if all communication with the upper part of the venous system and vascular system generally were cut off, experiment number one—with oxygen gas—was repeated,



with the following modification :—Before the injection of the gas into the cerebral cavity of the dead animal, a ligature was put firmly round the superior cava and round the aorta at the bases of those vessels. On injecting the oxygen into the cranial cavity the effect on the blood in the lower part of the inferior vena cava was the same as before.

*Observation 5.*

The above experiments were then changed in the following manner.—After the body of an animal which had slept to death in narcotic vapour had been opened, an open tube, or quill, was inserted and tied in the inferior venacava, the tube being made to take the direction of the vein. To the tube was attached a length of india-rubber tubing the further end of which was connected with an ordinary gas jet. The injecting needle, by means of another india-rubber tube, was now connected with a current of coal gas, and was inserted through the optic foramen into the cranial cavity. In an instant, as if nothing but a straight open tube intervened, the gas escaped so freely through the jet at the free end of the tube leading from the inferior cava that it could be lighted immediately. I made this experiment the matter of demonstration in one of my experimental and practical lectures in 1870.

*Observation 6.*

Hitherto I had used gases only for injection into the cranial cavity. I next determined to use a fluid, and selected mercury for the trial.

An animal of the same kind as before having been allowed to sleep to death in narcotic vapour, the abdomen was laid open and the inferior vena cava exposed full of blood. The needle was then inserted, in the manner



already detailed, into the cranial cavity, and, by means of a small syringe, a drachm, by measure, of mercury was injected through the needle. The mercury immediately appeared in the iliac veins and lower part of the inferior cava.

*Observation 7.*

In another experiment made immediately after the narcotic death of the same kind of animal, the injection of mercury into the cranial cavity was carried out without opening the abdomen more than just to expose the inferior cava at its lower part. The injection was very slowly pushed on, until from the cava the whole of the mercury ran through the venous system, and traversing the lungs through the pulmonary circuit, reached the left side of the heart and extended into the arteries. A complete general injection of mercury was thus established.

*Observation 8.*

In a succeeding experiment the details were carried out as in the last, except that coloured water was used for injection instead of mercury. Again the injection found its way into the inferior cava, but, owing to absorption by the nervous matter, not so abundantly as when an insoluble gas or mercury was employed.

At various times since the above-named observations were carried out, I have repeated them on other dead animals with the same results.

The injection answers in the same way in the dead human subject. In the process of embalming, four pints of the preserving fluid can be injected over the body through the subcutaneous injecting needle by the cerebro-spinal cavity.

Up to this time I have been quite unable to discover

how, from the cerebro-spinal cavity, the injected matter enters the veins so easily. It charges the plexuses of the medulli-spinal veins which lie between the pia mater and the arachnoid, but the entrance of it is obscure. No injury is traceable in the brain substance after the injection, unless undue force is used, but it is obvious that the lateral ventricles are first charged with the injection.

The late Professor Sharpey, before whom I once made the demonstration, recommended me to follow out, from a mercurial injection, the tracing of the injection into lymphatic vessels; and Mr. Gay, whose admirable researches on the venous circulation are so well known, suggested, also after a demonstration, the injection of a solution that would solidify in the vessels, and so permit of being followed by a minute dissection in the entire course through which the injection flows. There are two important anatomical inquiries which remain yet to be conducted.

#### PRACTICAL INFERENCES.

Meantime, from what has been so far observed, we learn that the chief point of exit for gases or fluids in the cerebro-spinal canal is by the veins of the canal into the branches communicating with the inferior cava at its lowest part. The fact has many practical bearings, inasmuch as it leads to the almost certain inference that the cerebro-spinal fluid, which is always being secreted, finds its return current into the venous circulation in a similar manner, and that the cord and brain are thus relieved of pressure from it. If that be so, we have a ready and rational explanation of many cerebral and spinal disturbances occurring under retarded current of blood through the vena cava inferior by pressure

on the vessel or by obstruction in it in some part of its course.

Here is an instance probably in point.

I was called to meet my friend Dr. Cooper Rose, of Hampstead, in consultation, in the case of a gentleman rather advanced in life who was labouring under ascites. The abdomen was tense with the fluid, but the urgent and apparently fatal symptoms were cerebral and spinal. The patient was quite unconscious, breathed with deep stertor, and had frequent convulsive starts and tremors.

I explained to Dr. Rose, from the view derived by the research I have described, that the nervous symptoms might be due to obstructed venous circulation and what might be called cerebro-spinal dropsy. Thereupon I also ventured to suggest that it would be good practice, notwithstanding the apparently hopeless state of the patient, to tap the abdomen and draw off the fluid.

Dr. Rose agreed readily, and we performed the operation at once. We had drawn off about eight pints of fluid, letting it flow through an india-rubber tube into a metal pail by the side of the bed, when, to our intense surprise, our patient raised his head and asked, as if awaking from a dream, "what it was that was running." He had no knowledge that he had been operated on until Dr. Rose gave him the information, and he could scarcely accredit that so large a quantity of fluid had been drawn from him. By the time his abdominal cavity was emptied of fluid this patient had entirely recovered his consciousness. He lived for nearly two years afterwards in comparative comfort, undergoing the operation of tapping several times, and sinking at last from gradual exhaustion increased by senile degeneration.

I think it probable that the vertigo which is met with

in temporary hepatic obstruction is often due to the cause I have indicated, namely, retardation of the return ascending venous current, and suppression of the cerebro-spinal secretion. I think it probable that hysterical convulsion, preceded by great distention of the abdomen from flatus, may be due to like obstruction from pressure. That puerperal convulsion should be mechanically induced in the same manner is no more than would be expected now that the mode of discharge of the cerebro-spinal fluid into the general circulation is seen to be, chiefly, at a part of the venous system where uterine pressure is most likely to prove an impediment to the ascending venous current.

## ON THE MAINTENANCE OF LIFE IN A FAC- TITIOUS ATMOSPHERE.

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**I**N the early part of November 1879, I was invited to the Royal Polytechnic Institution, to witness what was a remarkably novel experiment.

It was reported to me that a young gentleman named Fleuss had come to the institution and had asked permission to descend to the bottom of the great diving tank without the assistance of the diver or the use of the apparatus by which air was supplied to the diver during his daily descents, and that on the condition that a strong cord should be attached to him, by which he could signal if he were in distress, and by which he could be at once raised out of the water, he put on his new dress, descended by the ladder into the water, and remained under the water for forty minutes, independently of any assistance whatever. Every few minutes he tapped his metal helmet with a coin to indicate that all was safe.

At the next descent of Mr. Fleuss, I watched every step of the process. I was so thoroughly conversant with the authorities at the institution and with its working, I was sure there was no collusion, and when the experiment was over I knew that I had

witnessed the fact of a man existing twelve feet under water, at a temperature of 48° Fah., for nearly an hour without being injured.

I began at once to speculate how the thing was done. I put each position down in detail. (1) Fleuss must carry with him a vessel of condensed oxygen or air for air supply. That was probably in the helmet, which seemed shaped for such a purpose. (2) He must have a means for fixing the carbonic acid evolved by his breath. That was probably done by expiring over a surface of potassa or soda. (3) He must have means of dealing with the expired nitrogen, for he could hardly have made a discovery of a practical mode for fixing that gas, and certainly he let no bubbles of gas escape from him while under the water. (4) What he did probably was the ingenious plan of catching the nitrogen in a bag or reservoir and letting compressed oxygen loose into the same reservoir, making, in short, a factitious atmosphere upon which he lived during immersion.

On coming out of the water, Mr. Fleuss was introduced to me. I was struck with his singularly self-possessed and quiet manner. He was not in the least anxious respecting himself. He was only anxious about the fears of Mrs. Fleuss, who had accompanied him to the institution, and who was naturally under some apprehension as to the results of so novel a venture.

When I had quite made up my mind as to the way in which the experiment was carried out, I told Mr. Fleuss the conclusion I had arrived at. He admitted with the utmost good humour that he was found out, took me into his workshop, showed me all the apparatus, every bit of which had been constructed by his own hands, and begged me to make whatever observations I liked,



with the simple and honourable proviso that I should not divulge his process until it was duly protected.

I soon made a number of new observations which I have recorded in *Nature* and before the Society of Arts, at length. Convinced that the profession of medicine has not as yet had the opportunity of appreciating the singular and important physiological bearings of these experimental proceedings, I shall briefly summarise them in this paper. Before I proceed, however, to this task, it is necessary to describe the apparatus by which Mr. Fleuss is able to live in a factitious atmosphere.

#### THE APPARATUS.

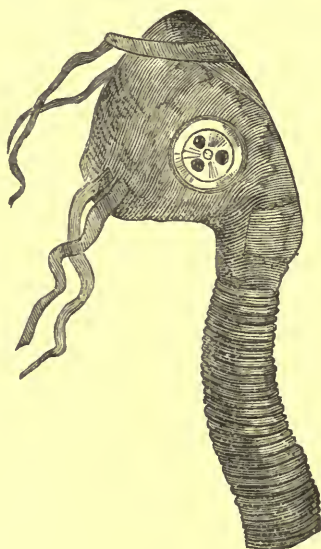
Mr. Fleuss' apparatus is now modified in order to suit different conditions. It is sometimes a diving, at other times a mining apparatus. Under all circumstances it is the same in principle. It permits the person who wears it to live under water or in a poisonous atmosphere without danger to life.

In the latest improvement the reservoir, which is carried upon the back of the diver or explorer, consists of a strong sheet copper cylinder, 12 inches by  $6\frac{1}{2}$  inches, with domed ends, and capable of holding 4 cubic feet of oxygen gas at a pressure of 16 atmospheres. Above the cylinder, and attached to the side of it, is a square metal box, 12 by 12 by 4 inches, to contain the carbonic acid filter, which is a box of vulcanite, divided into four compartments, with a wooden lid made quite air-tight by an india-rubber washer, and having an inlet and an outlet tube. The box is filled with a packing of ordinary tow, interspersed with two pounds of stick caustic soda. The exhaled breath passes twice up and down through the interstices of the tow, by which it is



finely divided, and thoroughly freed of carbonic acid by the caustic soda. The condensed water of the breath is collected under a perforated false bottom of the box.

A flat bag of vulcanized india-rubber fastened to the apparatus is connected by an india-rubber pipe over the shoulder to the outlet pipe of the filter. The bag is also in communication with the oxygen chamber, and the supply of oxygen to the bag is regulated by a jamb screw valve under the control of the wearer.

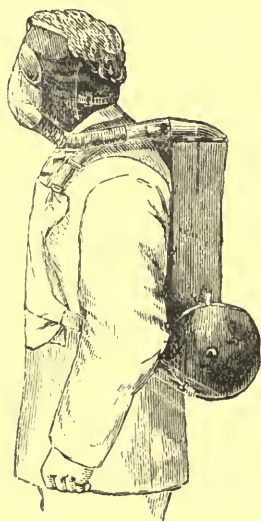


An india-rubber mask, constructed, as will be seen from the drawing, very much like Sibson's chloroform inhaler, is made to fit air-tight to the face, and is held in place by straps buckled up at the back of the head. The mask is fitted with a pair of flexible valved pipes, —the one for exhaling being in communication with the inlet pipe of the filter, and the other, for inhaling, in communication with the air bag.

The exhaled breath having passed through the filter

enters the bag cleared of carbonic acid and containing only the nitrogen and returned oxygen. There, the nitrogen meeting with its complement of oxygen, the factitious atmosphere is formed for inhalation. The bag, being perfectly flexible, readily expands or contracts with the breathing, so that no effort is required for respiration.

The apparatus is easily adjusted, is readily put on, and is easy to wear and breathe in. I have been in it myself, and have experienced, when in it, no more difficulty of breathing than in the open air. If any sense of closeness or oppression occurs, the admission of a little more oxygen, by turning the small tap, immediately meets the difficulty. The apparatus as adjusted is shown in the subjoined diagram, for which and for the previous one I am indebted to the kindness of the proprietors of the *Graphic*.



The oxygen reservoir is now carried, it will be seen, on the back, instead of being in the helmet as in the original design.

With these brief descriptions I may proceed to the experimental observations which it was my privilege originally to make. Three of these will suffice.

#### 1. EXPERIMENT UNDER COLD WATER.

On the 15th of November, 1879, Mr. Fleuss put on the diving dress at 6.33 p.m. His temperature was natural, 93.4° F. His breathing was perfectly calm and easy. His pulse was 72 small—naturally—but of good tone and regular. The sphygmographic trace was very regular

and equal. At 6.40 p.m. he descended into the water, the temperature of which was 35° Fah., three degrees above freezing point. He descended to the depth of twelve feet, and signalled to me when he reached the bottom. By preconcerted signal he was able to tell me how he was going on; and after remaining in the tank for an hour, he gave a signal to be drawn up, the coldness of the water having produced numbness of his hands. He walked up the steps out of the water carrying the heavy weight of 116 lb., which had been used to keep him down. The helmet and mouthpiece being removed, I found his breathing quite free, and his countenance rather pale, but perfectly natural. His pulse was 120, but soon began to fall. The mouth temperature was 92° F., but there was no rigor nor any sign of exhaustion. At the end of seven minutes the diving dress was removed and replaced by a warm suit. The pulse was now 90 per minute, the mouth temperature 94° F. Twenty minutes later still, the pulse was 80, the mouth temperature was 96°. During these periods I took a sphygmographic reading of the pulse. It showed a rather shorter ascending stroke than is natural, and a prolonged wavy descending stroke, but no intermission.

In a brief period after the last-named observations on pulse and temperature all the conditions became natural, without a trace of painful reaction.

## 2. EXPERIMENT IN CARBONIC ANHYDRIDE.

On the 25th of November I made another observation. I filled the diving-bell of the Polytechnic Institution with carbonic acid gas. Mr. Fleuss descended beneath the bell and then ascended into it, and sat in it for the period of twenty minutes, safely protected from the effects of the gas. He was not affected in any way by

that experiment. His temperature was  $98^{\circ}$  F. when he went into the bell, and his pulse 68; his temperature was  $97\frac{1}{2}^{\circ}$  when he came out, and his pulse the same as before, 68. While in the bell he made repeated efforts with lucifer matches to light a taper, but without effect.

### 3. EXPERIMENT IN CARBONIC ANHYDRIDE AND AMYL HYDRIDE VAPOUR.

I thought it would be well to try if vapours that were of a very much lighter character than carbonic acid gas could be tested in a similar manner, or whether they would in any degree permeate through the apparatus in such a way as to interfere with the success of the method. To test this, I charged the diving-bell, still containing carbonic acid, with an atmosphere of amyl hydride, a very light gas, resembling choke damp in its lethal action. *No more crucial experiment could be tried.* Mr. Fleuss entered the bell in this irrespirable atmosphere, remained twenty minutes, and then came out simply because his supply of oxygen began to fail. His temperature on going in was  $98^{\circ}$  F., and his temperature on coming out was the same. His pulse was 72 on entering, and it was the same when he came out. In a word, he was unaffected altogether by an atmosphere, the inhaling of which would have produced insensibility, probably fatal in character, in sixty seconds. A pigeon placed in a similar atmosphere without protection from it was insensible instantly, and was dead within a minute.

As it had never been the lot of any previous observer to see such results as are here described, I witnessed their performance with an anxiety not easy to explain, and with an admiration at the courage of Fleuss which is even more difficult to express. The cost of the anxiety

was repaid by the grand practical demonstration that a man in a deadly atmosphere can make for himself a factitious atmosphere, in which he can live and move and have his being, without any inconvenience then or afterwards.

I have repeated some of these demonstrations since at the Society of Arts, and Mr. Fleuss has repeated them in various practical ways. He has walked four hundred yards under water; he has entered chambers filled with irrespirable products derived from burning materials; he has entered choke damp in a mine. Already many lives have been saved by the use of his apparatus, the value of which every common sense person will discover without further description or comment.

#### THE PRACTICAL MEDICAL LESSONS.

It remains for me to refer to some of the physiological and medical readings of these experimental truths.

Amongst many such truths I gather the following.—

As the nitrogen and oxygen are only mechanically admixed in the breathing-bag of the apparatus, it is proved that for vital purposes a mechanical admixture of these gases is merely required.

Immersed in water close upon freezing point, Fleuss was able, in an atmosphere containing an excess of oxygen, to live an hour, although his temperature was reduced 6° Fah. below what is natural, and to return into a warmer medium without one bad symptom.

The fact is directly suggestive of a method of reducing pyrexia by inhalation of oxygen in the cold bath in extreme cases where the temperature is running up to fatal increment. At what precise time Fleuss registered a mouth temperature of 92° I cannot say. How much lower than 92° he registered in the water I cannot



say. I can do no more than state that seven minutes after he was out of the extreme cold he was 6° Fah. below his natural temperature and yet recovered safely and quickly. The inference is, I think, comparatively fair that had Mr. Fleuss been in a condition of fever equalling 104° Fah. when he entered the cold water, his temperature, within the hour, would have fallen to 98°. It is further, I think, a fair inference that by a modification in applying the cold, suitable to the sick, coupled with an easy plan for inhalation, we have a ready method of checking the fatal increment of febrile heat.

It is also open to inquiry whether in such a disease as tetanus the explosive paroxysms may not be controlled by the same simple process.

It is only fair, in concluding this paper, to say that, in 1854, a Frenchman named St. Simon Sicard made a diver's dress with a reservoir for oxygen gas compressed under six atmospheres, with an apparatus for absorbing the carbonic acid, and that he, too, lived in an atmosphere of oxygen mixed with returned oxygen and nitrogen. This apparatus was shown at the old Panopticon in Leicester Square, and Sicard, or some one representing him, was immersed in it under water. Unfortunately he did not make proper provision for the removal of condensed water of respiration, and he was, I have been told, all but suffocated, in one experiment, from this cause; whereupon the apparatus was condemned. Mr. Fleuss, by the removal of this source of danger, and by his independent skill and courage, has left the invention singularly complete.

## FELICITY AS A SANITARY RESEARCH.

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ANITARY science, in some simple directions, has recently won triumphs such as have never been won before, and its advocates, once called enthusiasts, dreamers, visionaries, and other poetical names, are now, in respect to enthusiasms, dreams, visions, looked upon as commonplace observers. The miracles they declared possible are performed so regularly that wonder and doubt have ceased.

We sanitarians declared that it would be a comparatively easy task to find out the courses and, at least, the proximate causes of the great pestilences, and that, with a fair knowledge on these subjects, combined with a comparative ready assistance by the public, we could control both the courses and the causes. The work has not been done so thoroughly as we could have wished, because the public has not come up, as yet, to our views; but the work is progressing, and sufficient is already done to prove the truth of our position.

We said that the once current rates of mortality were deathly of deathly; that they represented a low

*[A Lecture delivered to the Congress of the Sanitary Institute of Great Britain, at Glasgow, Sept. 27th, 1883.]*



civilization; that they might, in these countries, be reduced generally to a mean of fifteen in the thousand per annum, and, in favoured localities, to a lower figure still. We were taunted with the rejoinder, that if such were accomplished men and women would live a hundred and fifty years. We replied, Let them live two hundred years if they can, but let us, meanwhile, reduce the huge mortalities which are considered natural.

The result of our work is that there are towns where the average mortality is actually lower than fifteen in the thousand. Strangely, too, the popular cry is not now against us as enthusiasts, but against towns which do not follow up our enthusiasm. Towns, therefore, in this day, compete with towns for a low death rate, knowing well that should mortality, from temporary and as yet accidental causes, rise to what was considered the mere natural a quarter of a century ago, they are temporarily ruined if they depend upon outside popular favour for existence.

All this is most satisfactory, and would afford a fruitful theme of discourse. But what I want now to rivet on the memory is a new thought for new work. I want to put a question or two and endeavour to answer them.

Can we honestly believe that these triumphs of ours, which have so far ended in a certain victory over death, have introduced any great triumph over misery?

Have we by our labours made men, women, and children greatly happier as well as longer lived?

Have we tried to effect anything in that way, or have we, aiming at nothing more than the promotion of a longer life, left the rest to chance, as if it were not our duty to include human Felicity in our design of labour?

Can we effect anything to ensure Felicity as well as length of days? In other words, is Felicity a subject open to sanitary research, and if so, in what directions shall we labour for it?

These questions are momentous, because, if we are aiding in the art of adding to length of life and in developing populations, without giving to an extended and universal life Felicity, or the enjoyment of that which is given, we may, in the long run, be working evil rather than good for the human race.

A race unhappy lives too long to live.

Surveying the questions I have submitted, I do not think that we have, so far, done very much to add to human felicity. In the first stages of our labours that, indeed, were impossible. We have had to deal with very unpopular and, some think, unsavoury subjects. We have had to be excessively personal. We have been obliged to tell people to be clean both at home and abroad. We have been forced to be fault-finders all round. We have even had to frighten the masses, and fear is a terrible foe to felicity, both in the house and out of it; and until, one day, I ventured to show, by an allegory, a pleasant side-station on our steep and narrow road, we seemed to conceal the destinies we had in view, or to leave them for anybody to discover,—an almost hopeless leaving.

We have not tried, therefore, in any direct manner to teach the way to felicity. We may, like the rest of the world, have spoken of health and happiness. We may have commented on the sound mind in the sound body; but we have not tried to systematize effort towards the attainment of felicity as we have towards the attainment of length of days.

I may make these admissions without the slightest

compunction or regret. If we have done no good in the direction I have referred to we have certainly done no wrong. There has not been sufficient time for a development of Infelicity from extension of life, so we need not ourselves be unhappy.

But now comes the last question.

Can we by any future effort advance human felicity by a scientific research into the sources of it, and the impediments to it? Can we, scientifically, connect health with happiness? If we cannot we had better never have been born. We are like preachers of mercy who are empty of charity. We are mere sounding brass and tinkling cymbal.

I do not for my part believe that we are in this plight. I hope it is all right that we were born. I believe that we have the moral as well as the physical health in plain subject before us for study, and that we of all men ought to see how to combine the physical with the moral, and to understand the relationships of the one to the other and the interdependence of the one on the other. I trust, therefore, that we may soon take a new departure by inaugurating a new school of sanitary students and scholars, whose interests it shall be to learn the physical and moral art of living well, so that bodily health shall, of a truth, be mental felicity. A dream, do I hear it said! If I do, I hesitate not. It was a dream a quarter of a century ago that men could touch death rates and reduce them to order. To-day the dream is a fact.

I see no reason why we should not, by patient research, know all that pertains to our own lower natures at least. I can feel the astronomer overwhelmed with the sublime story that lies before him. I can see the metaphysical philosopher overwhelmed as he questions

the illimitable destinies and the sources of illimitable power and will and being. But we sanitarians dealing with secondary phenomena, with phenomena repeating at every moment, with our timepiece selves going through regular courses of eating, drinking, breathing, thinking, working, wearing, sleeping; with mechanism that can be counted, measured, weighed, and calculated on commercial values; we surely have no insurmountable difficulties to get over in determining what are the conditions under which human felicity is possible, and what are the conditions which prevent the accomplishment of felicity. The hardness of the task lies, at the outset, in getting a good view of the actual meaning of Felicity.

Felicity is contrast to misery. To many minds no more. A fancy, an invisible breath of some poet who writes what he has never known, and has never expected to know; or a laugh of some cynic who, tired of life and its vanities, declares that "All things are alike to all," and that man, with the beasts, neither goes upwards nor downwards, but dies.

To men who, like myself, are engaged for ten to eleven months each year listening to the sorrows which the sick are forced to tell and the healthy are forced to confirm, it would not be difficult to conclude that there is in the human world no such thing as felicity. There was a noted physician I once knew well, who told me at the close of a career of fifty years of active practice, during which few men had seen more of his fellow men or had observed more keenly, that he never had met a perfectly, nor, indeed, a comparatively happy human being. His view was not altogether peculiar. The professors of medicine, generally, are felt by many to be stoically indifferent to sorrow as compared with

other persons. They are not so at heart, but, knowing the smallness of human felicity, they are less oppressed than others by the extreme and tenderly acute occurrences of sorrow. They read the Book of Wisdom every day from nature, as the Chaldaic writer of it did, and so, in the everlasting presence of nature, become possessed of a demeanour which seems to separate them from the individual life; and as in that presence felicity is not the feature they are most wont to recognise, they give it wings to fly.

#### PROOFS OF FELICITY.

By the hard and fast scientific mode of looking at the phenomena of Nature, it might seem, then, at first view, that human felicity had no proofs of existence. There are, fortunately, other evidences which give positive proofs in characters as purely scientific as any in the observation of science. Granting that these are exceptional evidences, they are still in proof, and are within the knowledge of most observers.

I notice four of these evidences as all-sufficient.

1. In perfect childhood, uncrossed by perverse and chilling influences, and blessed by health, felicity exists, not, perchance, universally, but as a rule. I remember some few pages of my own childhood which were filled with an unbounded felicity—a felicity to be remembered, although it cannot be again realized or explained in relation to the precise causes that led to it. I have questioned others on the same point, and although the response was much more frequently in the negative than I expected to find it, and although the inquiry has often laid bare a recollection of misery rather than of felicity, that could not have been anticipated in child-



hood, it has yielded, certainly, a majority on the affirmative side. The evidence is sufficient to prove the reality of the phenomenon in at least one stage of life.

2. There are, again, men and women who, by some fortunate heredity of constitution, go through long, trying, and eventful careers with perfection of felicity. Dr. Joseph Priestley was one of these fortunates. "I was born," he says, "of a happy disposition." And so this man, through a life of struggle and tempest such as few men have known, was ever in felicity. In his child life he loses his mother. He leaves his home, and is domiciled with an aunt, whose gloomy tenets would drive some natures to the deepest melancholy. He passes through severe changes of thought on solemnest subjects. He becomes a preacher, but, owing to a defect of speech, cannot display an eloquence he knows is in him, and, tossed from pulpit to pulpit, penniless, is forced to teach that he may live. He becomes half friend, half librarian, of a nobleman, by whom he is petted at first, and then, with the capriciousness of power, is turned off, as a once favoured dog might be, without a word of explanation. He makes one of the grandest discoveries of the century, and lives to see the discovery accredited to another man, to whom he communicated it in the most open manner. Suspected of sympathising with children of liberty he becomes, under the instigation of a rival preacher, the victim of a furious mob, which burns his house and all his precious papers and treasures, wishing him heartily the same fate. Escaping to London he is obliged to hide from enmity, and, cruellest cut of all, is disowned by and cast out of the learned



society, whose work he has helped to immortalize. At last, driven in his old age from his native country, he goes, forgiving every one, to a foreign and distant land, to die there in perfect peace.

Such changes as these, such oppressions through every stage of life, would kill a multitude of ordinary men. Yet here was one who went through every phase of suffering with felicity. His friends, one and all, bear witness to this fact from their objective side. He personally testifies to the same, and explains the reason: "I was born of a happy disposition."

We gather from such instances as these, rarely it is true, but reliably, that in the range of physical life there is a felicity due to heredity; to some combinations of ancestry, which, being repeated, would lead to the birth of an almost new race amongst which Priestley's own maxim, "the greatest good for the greatest number," would be the common blessing. For, that which has once been born may be born again, and by birth become universal as a progress. If one man can hold felicity in his hand all his life and under all adversities, why not all men?

3. There is a third proof of felicity which comes within the knowledge of the majority of mankind, although it is not universal, for I have known a few who have afforded no evidence of it. This proof consists of the sensation felt, I repeat, by most persons of a sense of peace, tranquillity, and, in a word, felicity, which, in consequence of its abruptness and the sharp contrast between what has gone before, is a cause of extreme surprise. In such moments the actual cares of the world, cares heavy and sorrowful, sit lightly; the impossible, a short time before, becomes the pos-

sible or the easy. Dark forebodings which have pressed, almost to despair, pass away, and the future is roseate with prospect.

There are very few persons who have not experienced this curious change towards felicity. They may say that it is a fleeting change, and that may be true; but the fact is certain. It is, also, immeasurably instructive, for, if felicity can be obtained for one day, for one hour, why not for all days, for all hours?

These flashes of felicity are, I have said, sometimes startling from their abruptness. They are at other times equally startling from their intensity, and from the relief they give to the opposite depression, from which they stand out in contrast.

In speaking of this contrast, and of the advent of felicity after extreme depression, the common terms used to express the conditions are singular. The depression is almost invariably described as a physical weight, and felicity as the removal of a weight, which, like a physical burthen, has oppressed the body, and, in extreme instances, has bent it low. "He is bowed down with sorrow" is an expression as true as it is striking. Bunyan seizes on this physical truth. His pilgrim, while yet wanting felicity, is troubled with a burthen which weighs upon his back night and day, felicity coming when that burthen falls from his shoulders. The illusionist has here defined what he himself had felt, and hence the force of a description which every man and woman who has read Bunyan has, with very few exceptions, recognised. Felicity is lightness from burthen. The common folk call it lightness of spirit, light-heartedness, as being lifted up above the common fate of daily oppression and daily sorrow. The terms define the state.

When felicity is most absent, the sense of depression shows itself in other ways which indicate the physical process, and suggest the ponderable nature of something that tells on the body and on the mind. In worst states of depression the faculty of memory is often overburthened with labour of details, long stored up, which are remembered, re-arranged, and re-conjectured upon with painful and accurate precision. The thoughts undulate, and great waves seem to overwhelm another organization belonging to the man himself, yet lying afar back and obscured by these rolling tides, dark, dense, material, weighty. With felicity all these waves of deathly pressure pass away. The memory is charged with no recurring scene of sadness. The calculated difficulties do not appear. The organization which lies in the shade becomes brilliant, and the future hopeful.

These phenomena constitute both by reality and contrast what may be called the full-grown subjective proofs of felicity.

4. Lastly, there are certain objective proofs which lookers-on may observe if they will notice others, and which as independent evidences are perhaps the most reliable. A good perception of character and, if I may say, diagnosis, leads the looker-on to note and know the symptoms of felicity in others, for the symptoms are clear. In the wake of felicity the pulses are regular, tonic, free. The breathing is natural. The eye is bright and clear. The countenance, even in age, is youthful. The appetites are keen but orderly. The judgment is sound and joyous. The muscular bearing is firm, co-ordinate, steady; there is no indication of carrying a load on the back, nor of oppressive sinking exhaustion.

In the above few passages I have sketched out the phenomena of felicity. I have entered into no metaphysical subtleties in definition, but have rested on every-day experience, and having thereby, I think, afforded evidence of the fact of felicity, I pass to the thought how to extend this state, a thought which, according to my view, is eminently sanitary and practical.

#### PHYSICAL CONDITIONS INFLUENCING FELICITY.

To arrive at the true mode of working for this object we cannot do better than survey, in the first place, the conditions under which the phenomena of felicity, and of its opposite infelicity, are manifested.

##### *Atmospherical Conditions.*

By a sort of general impression the weather is believed to exert a peculiar influence for and against the phenomena of felicity. In this view there is undoubted truth. An *increase of the atmospheric pressure and a decrease* is each a cause of felicity. In ascending from valleys to moderate heights there is, up to a certain distance, a distinct effect of the kind. So definite is this action that I know of one person who, under some conditions, feels life is a load too hard to bear, but who, in a dry, bright, mountain region to which resort is often had, throws off the despair altogether, and lives a new life. In the nicely-adjusted balance of atmospheric pressure against animal circulation of blood, the circulation is relieved by a moderate removal of pressure. But if such removal be too great, if the organs of the body become congested from the removal, as they may be, the spell is broken.

The brightness of mind induced by removal of pres-

sure and freer circulation is, however, bound by other conditions. Dryness must accompany lightness of air to produce the state favourable to felicity.

There may, again, be conditions in which a slight excess of pressure may be conducive to felicity. In regions where the land is low, compared with the sea level, a slight atmospheric pressure may be advantageous. The air is usually drier under pressure, the wind bracing, and the vital organs charged with blood, conditions essentially favourable in low-lying districts to the communities that occupy them.

There are *electrical conditions* of the atmosphere during which felicity contrasts strongly and strangely with the depression incident to other conditions. My friend Mr. Hingeston, of Brighton, has very beautifully connected these varying states of atmosphere, from electrical influences, and these varying states of mind, with cloudland. He reads in the clouds the outward and visible signs of the mental state. The large white-headed cumuli that collect in clear bright days are rotary storms of hail, rain, or thunder, gyrating from left to right. Several of these gyrating storms keep marching onwards in alternate spaces, marshalled in vast circular array, and rolling round a circumference of bright translucent calm.

On the approach of one of these masses of vapour the mercury of the barometer first falls, and then rises with great rapidity.

The accompanying and residual state of the atmosphere is congenial to health. Now the debilitated experience favourable reaction, and the mind is serene and happy. The air in these moments is antagonistic of disease. With the breaking up and dissolving of these large cumuli there is electric action, and



most likely explosion, as the vapour is condensed into water.

The entire atmosphere changes ; everything is dull and grey ; the so-called dyspepsia prevails, the acid indigestion of gouty habits, the scrofulous indolent and pitiable host of "never wells."

Thus, continues my friend, the sensorial effects of the electrical fluid are proofs paramount of its pathological, physiological energy, and the various forms assumed by the vapours condensing or dissolving in the air,—clouds,—may be considered not only as picturesque beauties in the landscape, but also as criteria for judging of some of the most potent effects resulting from the operation of an experiment, silently and delicately performed upon the functions and sensations of animated beings.

*Cold and Heat* each play different parts in the production and reduction of felicity. A dry and sharp cold, what is called a bracing cold, exerts a gentle pressure on the surface of the body, which fills the nervous centres with blood, and helps to felicity of mind. A long and piercing easterly chilling cold checks circulation, robs heat, and produces even melancholic sadness. A dry genial warmth acts like a bracing cold ; a long warmth with moisture checks the vital action, and produces a degree of depression which may be as intense as that which is induced by prolonged exposure to cold.

*The seasons of the year* which are attended with least exhaustion of the body are those which favour felicity. When the exhaustion of the winter and depressing spring months has been removed by the warmth of a genial summer and autumn, the time is most favourable for serenity of mind. On the other hand, the exhaus-



tion of winter and spring induces depression, and is no doubt the cause of that melancholy which renders the months of April, May, and June the maximum periods of death by suicide.

*Purity of the atmosphere* is an unquestionable aid to felicity. The comparison of children living under differing circumstances is sufficient proof of this fact. Children in an open well-ventilated schoolroom, how different are they from those who are immured in the close over-packed dens which are miscalled school-rooms. The felicity of the children of the well-to-do who live out of doors, and of the children of the fields and open streets, compared with the felicity of those of the small trader whose back parlour is living room and playground; the felicity of the man or woman who leads an out-door life, compared with the felicity of those who live in the close office or workroom, how entirely different?

#### *Foods, Drinks, Narcotics.*

There are still other agencies which bring or which check human felicity, and which are as purely-physical in character as those above recorded.

There are substances which taken into the body produce strange contrasts in respect to felicity and depression. Foods well cooked, foods carefully selected, foods supplied in sufficient quantity to sustain the body equably in all its parts, but so moderately as never to oppress the nervous digestive powers, conduce to felicity in the most telling manner. As a rule, all agents which stimulate, that is to say, relax the arterial tension, and so allow the blood a freer course through the organs, promote, for a time, felicity, but in the reaction leave depression. The alkaloid in tea, theine, has this effect.

It causes a short and slight felicity. It causes, in a large number of persons, a long and severe and even painful sadness. There are many who never know a day of felicity owing to this one destroying cause. In our poorer districts, amongst the poor women of our industrial populations, our spinning, our stocking-weaving women, the misery incident to their lot is often doubled by this one agent.

There is another agent more determinate in its effects and contrasts than tea, and that is wine. I am a total abstainer, but I am, I trust, an honest observer also, and I confirm, from direct observation, the old saying, that "wine maketh glad the heart of man." If it did this and no more, I should say let the felicity of wine remain to the world. Wine, like the alkaloid in tea, relaxes, lets loose the channels of the blood; gladdens like the ascent of the mountain side; gladdens like the gentle atmospheric pressure which forces more blood on to the internal parts. But, and here, alas! is the rub. Carried a little beyond the right mark, the felicity from wine passes into folly, the folly into feebleness, the feebleness into stupor, and the stupor into a depression the reaction from which is the bitterest, the most persistent.

Tobacco is another substance used to produce abeyance of anxiety. Tobacco is said to soothe irritability without stimulation, but it leaves, in many persons, long depression, coupled, generally, with an appetite for a renewed indulgence in it, which becomes intense. The confirmed smoker, who can stand out against indirect effects, whose taste for food and whose digestive endurance are little injured, is kept, during the whole time he indulges, in the state of suspension. He does not enjoy felicity, but for a time experiences a relief

from infelicity. My own experience, on the whole, is opposed to the indulgence, and I tasted it for a long period of my life, as well as observed the effect of it on others. To the aged it gives, I confess, a negative existence, which, when the mind is not filled with choice or refined or cultivated pleasures, makes time less wearisome. To the man who engages in work of great excitement and of a mental kind it brings a joyless repose. But, on the whole, it is a bad and sometimes a fatally bad indulgence. I have once known a man die directly from the effects, and how many I have seen injured I cannot say, but a large number. Again, I have seen many much depressed by it; so that I dare not put it forward, at its best, as a promoter of felicity. The world, I must conclude, would be happier if smoking were unknown.

The habitual use of opium for the obtainment of felicity is of the same erroneous character. The opium smoker, the opium eater, tells us of certain dreams and phantasies which are, for a moment, felicitous wanderings of the mind. I have visited the opium dens to see the effects, and whatever the dream may be, subjectively, it presents to the observer no sign of felicity. The expression of the opium smoker is one of restless and intense anxiety. He looks like a man in a dream of misery. His eyes are joyless, his features contorted, his skin colourless or dark, his pulse slow and labouring, his breathing hard and heavy; and, when from the half struggling consciousness he wakes to reason, the dream he describes is too confused to be accepted as a dream of felicity. Then he falls into dejection, which deepens and deepens until the desire to return to the cause of the dejection is too overpowering to be resisted. To opium eating and to

the subcutaneous injection of morphia the same description, with some modifications on which I need not dwell, is perfectly applicable. From the use of such an agent as opium there can be no result of human felicity. There could soon be produced by an extension of the use a madder world than now exists, a more miserable ; a happier, never !

And this saying, according to my knowledge, extends to all narcotic substances. There are some, like methylic ether and nitrous oxide gas, which produce for the moment infinitely more refined felicity than those I have specifically named, but in the end the results are the same.

#### CONSTITUTIONAL INFLUENCES.

I have dwelt thus far on influences of a purely physical kind in their relation to felicity. I have put these influences in contrast as affecting the state of felicity, and I have touched on some agencies which are used to produce a mock felicity. I must move from these to a brief consideration of other influences of a different nature which affect us for or against our happy or felicitous existence.

#### *Temperaments and Heredities.*

There are some constitutional differences, determined by temperaments, which are of first importance. Of the four primary temperaments, the sanguine, the nervous, the bilious, the lymphatic, and of their relation to felicity, a volume might be written, and I have collected the facts relating to the temperaments of over a thousand persons towards such a work. I must not here touch, however, on any detail. I must be content to record, as a general fact, that the sanguine is, altogether, the happier temperament, but not always

the most sustained as such; that the dark or bilious is the least happy in early life, but is often, in later life, more serene; that the nervous is a varying condition full of ups and downs; that the lymphatic is, by a negative effect, the most even; that, amongst the twenty-four combinations of temperaments the sanguine lymphatic is the most felicitous in respect to physical pleasures and the bilious sanguine and the bilious lymphatic in respect to intellectual; that the nervous sanguine is the most irritable; and the nervous lymphatic the most helpless and miserable.

The moral influences and impressions affecting these natures are, from first to last, potent for good or for evil. In childhood the future history of the felicitous or infelicitous after-life is usually written. A few, born, like Dr. Priestley, of a happy disposition, fight through all adversity, filled with a magic soul of felicity, but they are very few, and are commonly, though I dare not, in the face of natural truth, say always, good. They, even in childhood, are not affected as others are. In the schoolroom as in the playground they are comparatively happy.

As a rule, the tendency to felicity or its opposite is planted in childhood. The parent, the schoolmaster, the schoolmistress hold the book not of learning simply but of fate. To the imitative mind of the young, absorbing all that the senses can carry to it and the nervous centres can retain, the character of the presiding mind, ever present with joy or sorrow, justice or injustice, love or hate, cruelty or mercy as its qualities, is the beginning of the end.

#### FELICITY AS A PHYSIOLOGICAL QUALITY.

Let me, as bearing on these matters, not diverge



from, but converge to, our present study, by referring to the position of felicity as a physiological quality.

Of the two living natures with which man is endowed and which distinguish him from the lower creation,—the pure animal and the pure intellectual natures,—felicity belongs to the animal nature. An intellectuality separating man from the animal would leave him beyond either felicity or infelicity.

Felicity, in fact, is not an intellectual quality. It is not centred in the brain. It is not a quality which a man can think himself into, or reason himself into, or directly will himself into. It is like the beating of his heart and the circulation of his blood, a vital process going on independently of his volition. He can by rude process destroy it for himself as he can for others. He can kill it for himself as he can stop the motion of his blood by stabbing himself to the heart, but still the quality is so independent of himself that he is often forced to be in felicity by things and acts and circumstances which his reason scorns. "Why does this fool of a book make me happy?" said a hard and suffering and miserable patient once to me as he pitched his "Pickwick" to the foot of the bed. "Yet it is the only thing that does, while all the time I know that such a set of asses as these Pickwickians could never possibly have existed."

By experience of what seems to increase felicity, to lighten misery, to make misery, we often confound felicity and sorrow with intellect. This is merely our own external looking upon external manifestations of internal phenomena which we know we cannot influence in the same way as we can teach a lesson or convey a fact, but which we strive to control because we think we must do something, even for the uncontrollable.



Felicity and Infelicity are not intellectual faculties, neither are they passions, neither have they any direct relationship to physical pain. They are distinct from intellect, passion, physical pain. They are the only true emotions. The man who is destitute altogether of felicity is not, of necessity, intellectual, passionate, destitute of passion, nor more nor less sensitive to pain than anyone else. The most intellectual may be the most miserable; the most silly and inconsequent may be the most blest with felicity. The worst instance of extreme, I may truly say harrowing misery, I ever knew was in one whose clearness and calmness of judgment was a subject of general admiration, but who had never, he told me, known in all his life an hour of felicity. The man most replete with felicity I ever knew was one endowed with no intellectual supremacy at all, and who was all through a long life a veritable child.

The centre of the emotion of felicity is not in the brain. The centre is in the vital nervous system, in the great ganglia of the sympathetic, lying not in the cerebro-spinal cavities, but in the cavities of the body itself, near the stomach and on the heart. We know where the glow which indicates felicity is felt; our poets have ever described it with perfect truthfulness as in the breast. It comes as a fire kindling there. No living being ever felt happy in the head: everybody who has felt felicity has felt it as from within the body. We know, again, where the depression of misery is located; our physicians of all time have defined that, and have named the disease of misery from its local seat. The man who is always miserable is a "hypochondriac;" his affection is seated under the lower ribs. No man ever felt misery in the head. Every

man who has felt misery knows that it springs from the body, speaks of it as an exhaustion, a sinking there. He is broken-hearted; he is failing at the centre of life; he is bent down because of the central failure, and his own shoulders, too heavy to be borne, feel as if oppressed by an added weight or burthen, under which he bends as though all the cares of the world were upon him to bear him down.

#### HABITUAL, SENSATIONAL, AND MORAL INFLUENCES.

There are numerous influences which, in an indirect way, tell upon felicity, for it or against it, as distinctly as those grosser agencies of which I spoke in the earlier part of this section of my lecture. To these I would now invite attention.

The influences to which I am about to refer are, in reality, purely physical in their action, although they are commonly known as habitual, sensational, moral, or mental influences.

I notice, in the first place, that felicity is always favoured by sufficiency of *rest and sleep*. Bad sleepers know no felicity; but they who in childhood and old age sleep ten hours, in adolescence nine, and in full age eight hours, out of the twenty-four, and that soundly, are mostly well favoured with felicity. They may be exposed to causes which are opposed to felicity, but even then the causes are feebler in action than they otherwise would be. I put sleep in the first place as an aid to felicity, because it comes first. I have no knowledge of any instance in which a person who slept well was altogether devoid of felicity. The beneficent action of sleep is, however, indirect. It is due to the physical and mental strength it confers on its favoured child.

*Strength of body* secures felicity. Persons comparatively weak of mind may, with a good physique, be happy; but very few who are weak of body have any long tastes of felicity. We may take it all round that the feeble of all ages are unhappy. It is a matter of common observation that persons who are so unfortunate as to be born deformed of body, though the defect be concealed or hidden, are not blessed with felicity. It need not be the deformity that causes the infelicity, for the deformity may be concealed. The bad health is the rooted cause. Let the defect be from accident happening to a body born of good stamina, and felicity may be as in others, despite the acquired defect.

Any sign of inherited weakness is an equal sign of lessened felicity, though it be marked by no physical defect. It has been long observed by physicians that persons who from early life show very large and prominent veins, and thereby a languid circulation of the blood, are never happy, while those of well-knit body are. The observation is true as steel. We physicians know that a sluggish circulation is incompatible with felicity, and that they who show this indication, by such local diseases as hæmorrhoids for example, are amongst the most depressed of those who consult us. We say of them that they suffer from arrested circulation through the liver, and doubtless, such arrest is depressing; but the arrest means physical exhaustion, and physical exhaustion is the root of the evil. When the circulation is sluggish, the liver is sluggish, and the brain is sluggish, and the nervous centres are depressed. In a sentence, whatever prevents physical exhaustion and sustains physical strength sustains felicity; whatever exhausts sustains infelicity.

When the sun of life is high,  
All is bright.  
When the sun of life is low,  
All is night.  
Thus we laugh and thus we sigh,  
Light and shade where'er we go.

*Physical work*, when it is carried short of exhaustion, keeps up felicity, while sloth destroys it. But the physical work that exhausts kills felicity. The argument extends to *mental work*. Moderate, wholesome mental work is the best of all aids to felicity next to sleep; it strengthens the mind, it softens grief, it lessens care. Carried to excess it is pernicious, and destroys all felicity. Cowper, the poet, was wont to say that no labour is so wearing as composition, and few men possibly felt more unhappiness as the result of mental exhaustion than he. But his was the fate of all who force the brain to daily or nightly repeated weariness.

The influences derivable from sleep and bodily power are purely physical influences, but there are others called *sensational*, which, through the physical power, have a potent effect for or against felicity. The sense of hearing has the most intimate connection with the vital or animal nervous system. The auditory nerve, as Dr. Bucke has shown, possesses many of the characteristics of an organic or sympathetic nerve, and we all know how many external vibrations which reach the ear affect the digestive system, producing sense of warmth in the body, appetite for food and feeling of felicity; or, on the other hand, setting the teeth on edge, destroying appetite, and giving rise to gloom. Thus things told affect quickly, often permanently, for good or bad. The sense of sight influences also, but

less intensely, and after a different manner. Sights gladden or dazzle, or pall, or sicken.

Not to extend the argument, we may be sure that the senses, as doors and windows through which influences vibrate into the animal organization, can scarcely be touched by external phenomena without conveying some influence that shall make or disturb or prevent felicity. When they convey beauty in sound or picture: when they convey variety; when they convey cheerfulness of act, and manner, and voice, and feeling; when they convey to the soul the idea or story of generosity, of courage, of purity of life and character, of prosperity; then they convey felicity, which, passing through the brain on its way, finds its seat in the vital centres on or near the heart.

With Felicity as a sanitary research for my theme, I have striven so far to indicate what may be called the physiological bearings of the subject. I have endeavoured to show that felicity is something that is of hereditary quality; that it is something made or not made by external agencies, over which we have little control; that it is something made or not made by many agencies, which we have under our control.

In these respects felicity stands precisely in the same position as health. In the widest sense it means health, is another word for health. Health is born, and is made and unmade by external agencies which as yet are out of human control. Health is made and unmade by numerous influences which are under human control. Felicity, similarly influenced, depends on the good working of the animal or organic system of life.

I could enter here into one of the most absorbing questions relating to the connections which exist be-



tween the lower and the higher human faculties. I could indicate how the lower and higher nervous centres, charged during life with a subtle ethereal medium, communicate with each other and with the outer universe, and how by the states of this refined inter-communicating bond or sphere, both health and felicity are moved by external pressures, by external vibrations, by external agents taken into the living organism, by products generated within the organism and diffused through its own atmosphere. But I leave this inviting subject for one more immediately practical.

#### PRACTICAL DESIGN FOR FUTURE WORK.

Over those atmospherical causes which have been noticed as influencing felicity, we can exercise as yet no direct action. At the same time, just as we can now divert the lightning from its fatal course, we can indirectly effect good. We can prevent, as far as our teaching is effective, the erection of human habitations in dank and humid places, below the sea level, or in dry and arid spots. We can protest, and if we are clear and reasonable in our demands we can successfully protest, against the construction of new towns on melancholic foundations, and can explain, so far, the choicest places for felicitous existence.

A full Report from some learned sanitarian, on this one subject, addressed to those children of enterprize who are colonizing the worlds that are to come, would affect, if it took root at all, all the generations of the men to come in those new regions, and to a large extent the felicity of the whole future human family. In Africa at this moment the seed of new life that is being sown will, largely, be seed of felicity or seed of



sorrow, according to the selection of the sites on which the new and great centres of life are constructed.

In this direction man himself may divert the operations of nature herself. He may change her surfaces as he cuts down her forests, or plants new forests, or alters the courses of rivers, or makes new courses, or fills up valleys, or lowers or raises mountains, or connects or disconnects oceans. With the mastery of the surface of the planet in his power, man may, in fact, make what regions he pleases for good or for evil.

The Earth is the Freehold of the Sons of Man.

Again, if the natural air which man did not make may be, to a large extent, utilized for felicity, how much more easy is it for him to remove the unnatural which he himself makes, so that instant advantage of what is provided for good may be rendered serviceable?

Here our voices should be heard in a tone not to be mistaken. We shut up our young in closest rooms of close towns; we shut up our men and women by the million in close shops and factories. Some one million of us in these islands who call ourselves, with ignorant irony, the ruling classes, shut up some twenty-five millions of the people with their wives and children in walled-up atmospheres, where atmospheric purity is unknown; where cold and heat oppress; where food is what can be got; where drink is what can supply a false felicity for a certain sorrow; where marriages re-establish misery; where good sleep is impossible; where physical strength is so impaired that a perfect body is not to be found; where exhaustion from work is the daily cross; where things and objects of beauty

are rare as angels' visits ; where in the selfish race to barely live generosity is impossible ; where in compressed homes purity of mind is a thing the purest can scarcely maintain ; where variety is replaced by the dead monotony of unchanging sounds for the ear to hear, and scenes for the eye to see ; where fear dominates over courage ; where hope has no chance ; where prosperity is so little known that the worn-out life has no expectation this side of the grave ; and where death is so busy that three die to one of the more favoured communities. We, one million, I repeat, shut up our twenty-five millions under these conditions, and wonder why those millions know nothing of felicity ; why they are peevish, reckless, melancholy, sometimes drunken, sometimes rebellious, and ready to run after any leader who shall promise to lead them into a happier sphere, however little removed from that in which they are. Wonder ! the wonder is how human nature can bear such a famine of felicity, and live as if it only lived to die.

To give the boon Felicity to these masses, we, not as revolutionists, but as laborious workers for them and the thing most wanted, have, I trust, come into existence not in vain.

It is better for this work that we should be as we are, men of science rather than men of politics. We are, then, in the first and true place. We are educating politics as well as men by what we teach, an education which, in the present dense state of political darkness, is the noblest work we can possibly execute, until those who rule understand human nature.

#### SANITARIAN EDUCATION.

In the direction of education the sanitarian teacher

should, I think, begin to study the psychological side of humanity ; what human nature can and cannot bear ; how much pressure can safely be put on humanity without danger of explosion ; how much felicity can be secured by removing that danger.

Beyond the task of inculcating what are the necessities calling for pure atmospheres within and without the body, we can, most appropriately, explain what agents taken into the body are for felicity. We can teach what temperance in all things effects in this direction. We can use our earnest will in declaring for abstinence, where nothing but abstinence is the safe line of conduct. We can denounce every indulgence that undermines and mocks and destroys the blessing. We can insist on cleanliness ; cleanliness of body, cleanliness of mind. We can show how flowers and plants grow for health as well as beauty, and why for both health and beauty they must join the home circle if felicity is to enter it.

These are all sanitary questions, and they all, as one, bear on felicity.

We may educate, again, in another direction. It has been shown that some exceptional men are born of a happy disposition ; and it might have been shown, on the clearest evidence, that multitudes are born of an unhappy, nay, miserable disposition. We could easily, by our researches, describe what are the lines of heredity for the happy and for the unhappy dispositions. We could, with this discovery in our hand, with certainty of being listened to and attended to, impress on the people the truth that marriages ought neither to be matters of chance nor matters of mere monetary convenience, nor, indeed, matters of mere insane, so-called, love ; but that the marriage tie,

extending its influence into the future, and being no bond and seal of diseased heredities, should be the bond and seal of a healthier and happier racial progress in every succeeding generation. Now that our women are, by good fortune as well as good policy, made legally masters of their own property, the sanitary question, involved in marriage, was never so likely to be one of scientific value as it is at this moment, and as it will be in coming days.

We can instil some further lessons. We can tell those foolishest of the foolish of the world who think that riches and idleness and power are synonymous with felicity, that no success, no wealth, no power, gained by torture of effort ever brings felicity.

We can teach forcibly and faithfully on a different topic regarding which we have a large amount of information collected. We can adopt and urge with all our power our veteran Chadwick's advice to those who are working to instruct the young, that it is the perfection of prudence first to live, then to learn. We can insist that, inasmuch as felicity is impossible under mental strain, it is fatal work to press on the young mind the excessive labour which is now, in all departments, making cram, cram, the footing for knowledge. Lastly, we can tell the adult man struggling for the bubble reputation that broken sleep, and disturbed brain, and wearied muscle, and labouring heart, can never exist with felicity: that *sanitas* and *vanitas*, separated by one letter only, are, as the poles, apart from each other; and that *sanitas sanitatis, omnia sanitas* will never be established until *vanitas vanitatis, omnia vanitas* is blotted out for ever.

## COTEMPORARY PRACTICE AND LITERATURE.

*"Every physician will and ought to make observations from his own experience; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together."*—

FRIEND.

WYNTER BLYTH'S COCHINEAL TEST FOR LEAD IN WATER.

**I** HAVE had the opportunity of seeing a new and simple test for lead, lately discovered by Mr. Wynter Blyth.

The test is performed in the following manner.—

A solution of cochineal produce by digesting one part of cochineal in a hundred parts of proof spirit is first made. An ounce of distilled water is next placed in a white porcelain dish, and ten drops of the cochineal solution are added and mixed so as to give to the water a distinct rose-red colour. An ounce of the suspected water is then treated in another white porcelain dish in precisely the same manner, when, if lead be present, a purple-blue tint is struck, varying, according to the dilution, from a purple-blue to a light pink-purple.

One part of lead in seventy thousand parts of water, or one grain in a gallon, gives a most distinct purple-blue colour under the test. With careful manipulation, one part of lead in seven hundred parts of water, or one grain in ten gallons, gives a characteristic purple-pink.



## THE NEW OBSERVATION ON MYXŒDEMA.

The new observation on Myxœdema derived from the operations for removal of the thyroid gland recently performed by Kocher, opens up a singularly interesting chapter in physiological pathology. That Kocher's patients upon whom he had operated for extirpation of the thyroid presented after a time symptoms of the same kind as those which distinguish Myxœdema is clear, and the inference is, thereupon, fairly deducible that the loss of the function of the thyroid gland in the animal economy is the cause of the symptoms. If that should turn out to be true, the part played by the gland is more distinctly understood than heretofore, if it be not discerned fully. It would seem that in every case in which the thyroid was removed in young subjects, the operation was followed by the cretinoid state with Myxœdema as one of the marked symptoms. The first grand point to be determined is whether the results of the operation, which seem undoubted, are due to the removal of the gland or to the injury inflicted on the organic nervous system by the process of removal. If it be proved that the removal of the gland is the cause, and the evidence seems to point in that direction, the whole question of the relation of the ductless glands to secretion is at once revived. Or, if it should be shown that nervous injury has been the cause, a hardly less important question, the relation of organic nervous function to secretion will be revived. And again, a third question may arise, namely, Are these ductless glands regulators of organic nervous function like storage centres of organic nervous power? Whatever comes of the research it must be valuable, and as a



diversion from the eccentric and hopeless speculations on the germ hypothesis it is a happy relief.

#### POWER AND SEDGWICK'S MEDICAL LEXICON.

The eighth part of this admirable work, bringing it up to ECT, is now in the libraries, and, with its fellows, is one of the most useful additions; it is, in fact, an addition which no medical library can possibly spare. The conscientious care, the labour, the learning which the editors, who by the way are practically the authors, have bestowed on their task cannot be too highly recommended. The work is more than a lexicon, it is a compendium of useful medical definitions and history. In this eighth part, to take one example, the term "*delirium tremens*" is not merely defined, but is traced to its origin in 1813 to Dr. Sutton, who applied it to "that form of delirium which is rendered worse by bleeding, but is improved by opium." All through the same kind of useful information is thrown in. To the terms *diphtheria* and *diphtheritic* five closely printed columns are supplied, in which all the varieties of the disease are defined.

The new Lexicon based on Mayne's original work would have gladdened no one more than Mayne himself had he lived to see it. I knew Dr. Mayne, and it seems but yesterday since he would come to me when he ran up to town from Leeds, to tell me how he was progressing with his *magnum opus*, and how much he was indebted to our learned friend, Dr. Alexander Henry, for assistance and correction. A laborious student, Mayne, who, like the poet Cowper, felt composition the most exhausting of labours, and who also, like the poet, always seemed to be sad. I am glad, after a long lapse of years, to remember, that full of youthful hope and

energy at that time, I would cheer him up by telling him how his *magnum* would be sure to live and go through many editions, and carry down his name, and would see him depart with a gentle smile on his anxious face, and with his mind, for a moment at least, lighter. I am equally glad to see that my predictions were neither false nor magnified. But then it is not every author who has the good fortune to get two such scholars as Messrs. Power and Sedgwick to succeed him and sustain his inception.

#### RECORDS OF FAMILY FACULTIES.

Mr. Francis Galton has just published his outlines for *Records of Family Faculties*. He offers £500 for prizes to those British subjects, resident in the United Kingdom, who shall furnish him, before May 15th, 1884, with the best extracts from their own family records. These extracts, which will be treated as confidential documents, are to be forwarded on forms which may be obtained of the publishers,—Messrs. Macmillan, of Bedford Street, Covent Garden,—and which are put together in a handsomely bound volume. The design is so good it is to be hoped that, independently of the prizes, a large number of contributors will send the life histories asked for, especially those who “care to forecast the mental and bodily faculties of their children and to further the science of Humanity.” The prizes offered will be in various sums, ranging from £5 to £50, and a list of the gainers will be posted to each of them. The list will also be published in the daily journals and in at least one clerical and one medical journal.

#### NEALE'S MEDICAL DIGEST.

The last edition of Dr. Neale's *Medical Digest* is an

admirable and useful library companion. I find it of service every day, but never so truly as when, in the course of practice, I want to refer in a difficult case to other experiences than my own. Under such circumstances to take down the *Digest* and turn up the name of the disease is to see the way, often directly, to all the modern literature bearing upon the subject, a great saving of time and trouble.

#### POISON LORE.

Mr. Wynter-Blyth's new work on *Poisons, their Effects and Detection*,—published by Messrs. Griffin and Company, of Exeter Street, Strand,—is a handsome volume of seven hundred and twelve pages. It is essentially a practical work, forming a capital manual for the use of analytical chemists and experts. It is also conspicuous for literary research and manner. The introductory essay on *Poison Lore* may be read by scholars of all classes with interest; and if our fiction writers, who like to deal with the romance of poison, do not steal from it pretty freely I am no literary prophet. Like all the works published by Messrs. Griffin, the volume is beautifully got up and illustrated.

#### L'HOMME ET LA NATURE.

The book bearing the above title, by Dr. Hugh Doherty,—published by Messrs. Trubner, of Ludgate Hill, is the work of an author who, thinking for himself, combines much physiological learning with a wide knowledge of the great social problems of the present time. The cosmic unity of the universe, the individual unity of man, and the collective unity of the kingdoms on our globe, are syntheses of creation of

which the author attempts to make some analysis in order to discover the nature of the indestructible forces, their modes of movement, and the invariable laws of these modes.

DR. SEYMOUR J. SHARKEY ON MORPHIA HABITUÉS.

Since the reading of my paper on Morphia Habitués and their treatment, Dr. Seymour J. Sharkey has published, in the *Lancet* of December 29th, an admirable report of a case of unusual interest. The patient, who had injected himself with solution of morphia for a period of seventeen years, and who, according to his own statement, had for "a considerable time" taken 24 grains daily by injection, was cured, without danger, in ten days under the abrupt method of treatment, with some substitutional aids. The patient during recovery had symptoms of a marked character, but less intense than are sometimes seen when smaller doses of morphia have been used. The fact suggests to my mind a doubt as to the 24 grains a day for "a considerable time," and the report does not alter my view on the subject of treatment. Under the plan I have suggested the patient would, I think, have recovered quite as completely in ten to fifteen days without feeling the shock, which Dr. Sharkey so graphically describes as "the brake," and indeed, comparatively, without any suffering of moment.





ANEURISM OF THE CÆLIAC AXIS:  
A CLINICAL STUDY.

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N the twenty-second of November last year, I was called by my friend Mr. Frederick Hall, of Jermyn Street, to meet him in consultation in the case of a gentleman (Mr. T.) fifty-four years of age, who was confined to his bed and presented signs of great physical prostration. The intellect of the patient was clear and even vivacious. He was a very intelligent man, by profession a veterinary surgeon, and his knowledge of comparative anatomy and physiology made him more than usually interested in his own case, concerning which he speculated with considerable acuteness and judgment. He was emaciated, and his face bore signs of severe suffering, but at this moment he was free from pain. He was of nervous temperament, eyes grayish blue, hair brown, complexion fair. He was five feet seven in height, and naturally of good build of body.

PRELIMINARY HISTORY.

The *heredity* of the patient was of fair quality. The rheumatic diathesis had been developed in some branches



of his family, but tubercle and cancer were not known on either side of his parentage.

His own mode of life had not been prudent in respect to health. He admitted that for many years he had partaken rather largely of alcohol; had hunted regularly; and had, as he said, "lived a very free and active life."

He had suffered from previous illnesses, chiefly dyspeptic, many times, but had not been confined to his bed from that cause. From accidents by hunting he had been a severe sufferer, and seven years ago had sustained a fall in hunting, which caused fracture of three ribs on the left side, followed by pleurisy and a long illness. He dated his present symptoms from that accident. He "had never been well since."

#### SUBJECTIVE SYMPTOMS.

*Pain* of the acutest character was the first symptom elicited. The pain always commenced in the abdomen above the umbilicus. It was concentrate at that spot, but it extended in various directions, along the sides of the chest, into the loins, along the lower limbs even to the feet, and sometimes, but not often, into the upper limbs. The pain was "lanceolating neuralgiac" (I use the words the patient used), and when it was on it showed little intermission, until it entirely ceased, which it often did suddenly. When the paroxysm was on, the friends, as well as Mr. Hall, told me that the pain reached agony. The face was pale and bedewed with perspiration; neither counter-irritation, nor friction, nor soothing embrocation gave any relief. Position gave some relief. When he sat on the floor with his lower limbs flexed on the abdomen and his arms tightly clasped round his knees, he often, after a time,



checked the paroxysm, and in the attitude named would fall asleep, and sleep for many hours.

This description of the pain had so little variation, it may be taken as typical. The attacks rarely subsided in a shorter period than six hours. They showed no decisive periodicity. They were sometimes more frequent in the early part of the evening or night, but to this rule there were many exceptions.

The appetite was variable. It might be said that there was always some desire for food at proper times; but a frequent nausea, amounting often to extreme nausea, and increasing to vomiting, soon after food was taken, made the patient dread feeding. When the food was returned, it was, in nearly all cases, immediately after it had been swallowed, and when it was little digested. Sometimes the taking of food excited the pain, at other times it seemed "to ward off" the pain; there was no absolute rule.

The *mental power* was variable. Mr. Hall, in an admirable journal which he kept of the case, remarks that after the paroxysm the mind of the sufferer was sometimes "unhinged," so that he talked vaguely and without the intelligence that was natural to him. To me at the first visit he showed no depression and no wandering, but he was careful to inform me that I saw him under favourable circumstances, and that Mr Hall would tell me a very different story, for there were times when he "could not positively contain himself."

The *physical powers* were extremely reduced. The muscular system was much attenuated, and the pain took away the nervous activity. He was able, nevertheless, to get out of bed without assistance, and he showed me the mode in which he "doubled up on the floor" in order to obtain sleep.

## OBJECTIVE SYMPTOMS.

The *temperature* of the patient at the time of my visit was quite natural. I took it twice from the mouth and once from the axilla, and found it natural, 98·4 Fahr. Referring to Mr. Hall's daily report, I discern that this same condition was maintained pretty generally. There was now and then a slight febrile exacerbation, but never more than from one to two degrees, Fahr., of fever, and that at no time for longer than an hour or two at most. The patient's own observation was that he had no fever and rarely any thirst.

The circulation presented the most remarkable irregularities. The pulse, now seventy-eight in the minute, was large, soft, easily compressible, and at times jerky. Tested by the sphygmophone, the third sound was wanting at the radial pulse, the brachial, and the temporal. Percussion over the heart gave no indication of enlargement of the heart, but at the base there was an extremely loud, rough murmur with the second sound, and towards the apex a faint but prolonged systolic murmur. The rough murmur at the base was extended along the aorta into the carotids, and through both subclavians into the brachials. In the lower part of the body the murmur, very loud and coarse, extended along the whole of the abdominal aorta in the front of the body, into the iliacs, into the femorals, and even into the popliteals.

Connected with the arterial pulsations there was, in the abdomen, a special pulsation, to which the patient referred as the centre of all his distress and all his suffering. Immediately above the umbilicus there was felt a round pulsating tumour about the size of a fair-sized orange. The tumour was so loose that it could

be moved from side to side; pressure upon it did not produce pain, and yet it was described as the seat of all the pain. It pulsated regularly with the systolic stroke of the heart, but it retained its size after as before that stroke. At the period, when I was making the examination, the pulsation was easy and bearable; but when the paroxysms were on, the pulsation of this mass was said to be excruciatingly painful. The pulsation was visible through the bed-clothes, and all the large arteries seemed to be pulsating in unison with it. The pulsation in the carotids was so intense it caused a confused surging or beating sound in the head, which the patient compared to the revolution of a mill wheel. In this state sleep was impossible.

By making firm pressure on the common iliacs, I found it possible to stop the pulsation of the tumour, and I suggested such pressure as a means of treatment, with some temporary advantage to the sufferer.

The respiratory murmur, generally, was natural. On the left side there was some enlargement of four of the ribs a little in front of the angle, at the points where the ribs had been fractured, and here there was a limited region of dulness on percussion with deficient respiration. Apart from this, the respiratory murmur was natural throughout. There was no cough, except an occasional reflex cough during or after a paroxysm; and there was no expectoration.

A careful examination of the abdomen failed to detect any enlargement or misplacement of any one organ. The body being much emaciated, I was able, when the abdominal muscles were well flexed, to trace out the organs with fair precision. The stomach afforded no indication of scirrhus; the liver seemed to be smaller than natural, but not cirrlosed nor indurated; there was

no sign of intestinal thickening nor adhesion, none of mesenteric disease.

The general signs of disease of the great abdominal viscera were absent. The bowels acted with fair regularity, and the evacuations were quite free from any undigested fatty matter or other symptom of visceral derangement, except a frequent, "deficiency of bile."

The kidneys acted irregularly. Sometimes the urine was scanty and high-coloured, at other times pale and free; but Mr. Hall, whose watchful care was unceasing, could never at any time detect albumen or other abnormal product that would suggest the idea of kidney disease.

There was nothing in the character of the skin to lead to suspicion of disease of the supra-renal capsules. The skin acted naturally: it was moist without perspiration. Only during an extreme seizure of abdominal pain and arterial pulsation was there perspiration, and then it was the cold, clammy perspiration of faintness and pain; condensation of excreted moisture, not active exudation. There was neither anæsthesia nor hyperæsthesia of the skin.

The nervous phenomena presented were all distinct from anything indicative of spinal or cerebral lesion. All the nervous phenomena were sympathetic; and all were traced by the patient to the abdominal centre, in which, as he said, they took origin, and from which they, as it were, radiated. There was no indication of paralysis anywhere.

#### DIAGNOSIS AND PROGNOSIS.

The diagnosis in this case had been variously stated by different observers. The diagnosis of aortic aneurism had been suggested, but disputed on the ground that the

arterial murmur was quite inaudible along the vertebral column posteriorly. Pulsation of the aorta on a floating tumour had been diagnosed. Pulsation of the aorta on an indurated liver had been diagnosed. Mr. Hall had throughout believed that the tumour was aneurismal, but could not determine the precise seat to his own satisfaction.

After a long and careful examination of all the facts, on the exhaustive method, I was myself led to the conclusion that the pulsating tumour was an aneurism of the cœliac axis.

I came to this conclusion for the following reasons:—

1. All the symptoms pointed to the circulating system as the seat of the mischief.
2. The position of the tumour was at the position of the cœliac axis.
3. The well-defined and movable character of the tumour disassociated it, in my mind, from the aorta itself.
4. By careful auscultation with the differential stethoscope, I discovered that the pulsating stroke of the tumour, the lift, was not absolutely synchronous with, but followed, the aortic pulsation.
5. When the body was well flexed, I could grasp the tumour very fairly in my hand; could steady it; could feel the pulsation of it at the points where my fingers and thumb pressed it; and could, by gentle, firm pressure, control the pulsation.
6. All the other abdominal viscera were to my touch free of induration or irregularity. I considered the liver smaller than normal, if anything. The stomach, rather distended with



gas, was soft and compressible. The intestines gave no indication of disease or deposit. The condition of the alvine discharges gave no evidence of pancreatic disease. The renal fluid gave no suspicion of kidney affection. The colour of the skin afforded no sign of supra-renal affection. There was no indication of splenic enlargement. There were none of the disturbances of the canal nor peritoneal pains which would warrant the assumption of mesenteric glandular inflammation or enlargement.

7. The sympathetic symptoms were, it seemed to me, those which would naturally follow upon the presence of a pulsating tumour in the immediate neighbourhood of the great solar plexus.

The note I made at the time and communicated to Mr. Hall was, an aneurism of the cœliac axis, of the size of a cricket ball, as the principal affection, and as the cause of all the nervous paroxysms.

To this I added, from the heart sounds and general arterial murmur, disease—thickening and probably ossification—of the mitral valve, and dilatation with ossification of the ascending part of the aorta.

I also added, as the *prognosis*, a continued but perhaps prolonged exhaustion, with persistent sympathetic irritation, and probable sudden dissolution from rupture of the aneurismal sac.

#### TREATMENT.

The treatment which had been carried out in this case had varied in detail, but had been chiefly directed to the alleviation of the severely painful paroxysms and to the production of sleep. When nausea or vomiting



occurred, cold iced drinks had been given, with stomachic sedatives.

As a matter of experience, Mr. Hall had found that codeia, in doses of half to one grain, was the narcotic which gave the most relief with the fewest disadvantages. He had also found that a mixture containing hydrocyanic acid with digitalis had proved the readiest general sedative, and had seemed to control the pulsation most effectively. I recommended thereupon that these remedies should be continued.

I further suggested that a soft pad should be adjusted to the abdomen, and sustained by a light bandage round the body, so as to keep up a gentle and even pressure. When the paroxysm of severe pulsation occurred, I advised flexion of the thighs upon the body, with pressure upon the iliacs, firm and continued, until the pulsation was reduced.

The dietetic rules that had been enforced were so simple and plain they admitted only of being sustained. The food taken each time was in small quantities and of the most digestible character. Stimulants and all things which excited pulsation were withheld.

On December the 7th I visited the patient again with Mr. Hall. The report was that for a few days after my visit on November 22nd he had been more composed, and that the gentle pressure had controlled the pulsation considerably. About the 1st of December, however, the symptoms had returned with the same violence as formerly, and had been uncontrollable. Some chloral hydrate, which had been given with the codeia, had afforded temporary relief, and had led to sleep, but the reaction from it left a doubtful result.

There were now some new symptoms. The pupils were often much dilated, and responded sluggishly to

light. At times there was a restless comatose condition, with the mind occasionally wandering, and with a singular vibrating tremor of the muscles. On waking up he saw bright-coloured objects floating in the room, as if so many birds were flying from one part to another. With this there was a low, muttering delirium, followed by some excitement. But none of these symptoms were due to alcoholic stimulation, nor to any other assignable cause except the persistent pulsation in the abdomen.

The appetite by this time had become capricious to the last degree, food being, as a rule, objected to altogether. The sense of taste and the desire for food were alike impaired, and an extreme languor of mind as well as body attended the conscious periods of life. To me Mr. T. spoke for a while collectedly, and even cheerfully, still interested in the question of diagnosis, and anxious to impress on me his subjective symptoms. After a time this clearness of mind subsided into a wandering statement, with excitement.

In consultation at the present visit our thoughts were directed rather to the best mode of administering food and sustaining vital warmth than to any particular line of medical administration by drugs. It had been suggested that the narcotics that had been given might be a partial cause of the mental aberration. It was determined, therefore, that for a day or two they should be suspended.

The effect of the suspension of the sedative narcotics was not so favourable as to suggest their permanent withdrawal. The mental excitement, emotional and passional in type, continued, and with increasing waste and exhaustion was more pronounced. To secure sleep the codeia was resumed, and with marked advantage.

It allayed the restlessness and secured repose sometimes for three or four hours at a spell. The pulsation meanwhile continued, but in a subdued form, indicative of general advancing atrophy and exhaustion.

With little variation the symptoms were continued to December 15th. On that day, after a very restless night, Mr. Hall found his patient vaguely but piteously complaining of the vast number of "spectra" which he saw around him, and of a sensation in the skin as if "myriads of black insects were creeping out of the pores of the skin and covering the bed and dress." He was constantly striving to catch and kill these insects. Sometimes he would shout loudly, then he would mutter, and after a rest would become fairly composed, easy in his mind, and rational. For food and drink he had little care, but would take what was offered to him, and would often seem to digest it well. His bowels, inclined to be constipated, acted readily to simple laxative remedies. The excretions presented no marked peculiarities.

On December the 27th, after nine hours of a disturbed sleep, semi-coma rather than sleep, the patient rose suddenly, and for a short period was violent. He then became quite calm and natural. The pulsation continued, but not so vehemently. The abdomen was much collapsed, and the pulsating tumour, more easily defined, appeared to be rather increased in size.

In the latter part of this day, December 27th, the bowels were relaxed, much dark and offensive fluid being evacuated. This was attended with jerking pulse, eighty to ninety per minute in stroke, with faintness and, for the first time, a variation of animal temperature, a fall to 97·3. Afterwards there was some mental excitement, followed by calmness and

clearness of mind, but great lassitude. On the two succeeding days the bowels were not much disturbed, but on the 30th there was action with distinct clots of blood and returning symptoms of coldness and syncope. On the 30th, and for five following days, the hæmorrhage from the bowels increased, at first in form of coagula, but later as fluid dark blood in small but continuous streams, the pulse rising to over 120 in the minute, and the animal powers declining. Death took place without signs of suffering or of consciousness at 10.45 on the morning of January the 4th, 1884.

#### POSTMORTEM EXAMINATION.

On the morning of January 5th, 1884, twenty hours after death, Mr. Hall and I, assisted by Mr. W. Proctor, made a postmortem examination.

The muscular emaciation was extreme.

#### *The Thorax.*

On opening the thorax we observed at once the point on the left side where the ribs had been fractured. The fractures were well united. The lung was attached by three separate bands of pleural membrane to the ribs at the fractured part, but the structure of the lungs beneath was not injured, and the lungs throughout their entire structure were healthy.

The heart was of natural size and in its proper position. In the right auricle there was a loose fibrinous separation, and in the auricle and ventricle there was a little loosely clotted dark blood. The walls of the right ventricle were thin, but the valvular structure was free from disease. The pulmonary artery was rather dilated, but elastic, and its semilunar valves,

somewhat loose and flabby, were free of thickening or deposit, and acted correctly.

In the left auricle, as well as in the left ventricle, there were some loose masses of separated fibrine, soft, gelatinous, white. The walls of the left ventricle were thin but, in proportion to those of the right, normal. The mitral valve was extensively thickened in its anterior curtain, and the surfaces of it were rough from sub-endocardial calcareous deposit. One point of this deposit, standing up through the membrane, was sharply and firmly pointed. The coronary arteries were dilated, rigid, and patent. The aorta from its commencement was much enlarged. The aortic semilunar valves were thickened, roughened by calcareous deposit, and very imperfect in action. The walls of the aorta, devoid of elasticity, were charged with calcareous deposit, which projected from beneath the lining membrane so as to present a roughened surface which extended into the ascending arch, the innominate artery, the crown of the arch, the descending portion of the arch, and, in patches, through the whole of the descending vessel down to its division into the common iliacs.

### *The Abdomen.*

The abdomen was free of fluid, and the peritoneum showed no sign of inflammation, recent or remote. The stomach lay in its right position. It contained a few ounces of fluid, and was blanched on its inner surface. It showed no trace of organic disease. The liver, smaller than natural, was pale in its structure, but free from any organic change. The pancreas was healthy. The spleen was of natural colour and shape, but so reduced in volume that it weighed no more than five ounces. The kidneys, slightly large, were free of



organic change, and the supra-renal capsules were of natural consistency, size, and appearance.

On raising the intestines, the mesentery and upper surface of intestines being quite healthy, we came upon the tumour, the pulsations of which during life had caused so much difficulty. With very great care we dissected it from all surrounding superior structures. We found it to be firm, very round in shape, and of the size of an ordinary cricket ball. It lay nearly on a level with the umbilicus at its lower margin, and it inclined to the left side of the body, resting partly on the left crus of the diaphragm. It was attached superiorly to the aorta, and by its under surface to a portion of the descending colon.

In order to trace the connection with the aorta, we laid open that vessel and divided it vertically until we nearly reached the great cœliac branch. We then divided the aorta horizontally one inch above and two inches below this point, and were able now to lift the tumour up with the attached intestine which we tied and divided in about three inch lengths from the adhesion of it with the tumour.

The tumour, on being examined, was found to be a large aneurismal pouch of the cœliac axis, the dilatation being in the downward and forward direction from the aorta, and very equal. Near the point where the mass sprang from the aorta the three great branches of the axis, the gastric, hepatic, and splenic, were all so clearly traceable that we could pass a probe through each.

On turning the tumour over to look at the surface of it that was attached to the intestine we found a basis of union between it and the intestine as large as a crown piece; and when the intestine was laid open, we discovered, after removing some clotted blood, an opening



from it into the tumour, through which the handle of the scalpel would pass with readiest ease. There was some decomposition at this part. Replacing the tumour with its anterior surface upwards, we laid it open by a free incision. The interior was filled with a large firm fibrinous mass, strongly connected at one point for a space of about a florin, to the inner surface of the aneurism, but in all other parts surrounded with semi-coagulated blood which escaped through the opening connected with the intestine. The space of adhesion between the fibrinous mass and the wall of the aneurism was in the lateral surface on the right side.

The coats of the aneurismal sac were thickened, but except at the point where they had given way into the intestine, were unbroken. They had lost elasticity, but were not charged with calcareous deposit. There was no dissection or separation of the coats from interposing blood.

The tumour weighed nine ounces.

#### BRIEF COMMENTARY.

I record this example of aneurism because of its rarity and the peculiarity of the symptoms by which it was marked. The symptoms were so distinctive, they make diagnosis of the cæliac axis extremely simple. They afford ground also for much clinical study.

The intensity of the paroxysmal symptoms was due, no doubt, to the irritation of the centres of the vital nervous system, probably of the great solar plexus, which would be largely involved in the disturbance. Under this irritation the pneumogastric nerve lost its controlling power over the heart, either from failure of its func-

tion, or from being overcome by the over-action of the vital organic nervous system.

A very remarkable feature of the case was that the pulsation in the tumour was dominant over all the other arterial pulsations, and even over the motion of the heart itself. This is of interest in connection with the phenomenon of epigastric palpitation, in which the palpitation, acute, sickening, and painful, is in some rare instances—one of which I have recorded—disconnected entirely from cardiac palpitation or cardiac over-action.

The other nervous phenomena are equally singular. The dilated pupils, even under the influence of the opium alkaloid codeia; the peculiar symptoms resembling those of delirium tremens, *sine* alcohol; the coloured visual vibrations;—these, are all significant of the disturbance in the vital centres.

The carelessness and distaste for food is also an indication which should not be forgotten. That symptom is, I think, specially descriptive of nervous derangement of the centres of organic life. Recently I have seen a case of malignant tumour connected with the descending colon immediately below the curve, in which this entire distaste for food and many other symptoms observed in Mr. T., covered the malignant symptoms and made the diagnosis extremely difficult, and, to the end, doubtful.

From all the facts I could gather, I should infer that the aneurism commenced at the time of the accident in the hunting-field seven years previous to the death, and that the progress of the dilatation, slow and continuous, gradually lessened the vascular supply to the spleen and liver, under which those organs underwent reduction in size. The atrophy of the spleen was the

most complete I have ever seen. The organ was not larger than it would have been, naturally, in a child of six years of age. At the same time it was of perfect shape, colour, and consistency, and its structure throughout was quite normal. It is deserving of further observation, in connection with this atrophied condition of the spleen, that there was no symptom during life which in any way indicated splenic disease. Certainly there was no external sign of leucocythæmia.

In matter of treatment, all of us who were consulted were unable to do more than suggest palliative measures. I hoped that by *digitalis* we might control the cardiac action, and I have no doubt we did; but we could not thereby materially affect, for the better, the pulsation in the axis. The number of pulsations there followed the pulsations of the heart, but the intensity did not. That was vehement when the cardiac action was moderate or even feeble. In the opinion of Mr. Hall, who for many weeks was observing the phenomena daily, and often twice daily, the drug which rendered the most important service was *codeia*. Under its influence the patient always expressed that he received the greatest relief. It soothed pain and controlled the paroxysm more effectively than the other remedies. It produced no excitement in the first, and very little depression in the after stage of its action. In these respects it contrasted most favourably with the chloral hydrate, which was a few times prescribed.

## UPPER LONDON :

*A PROJECT FOR RADICAL RECONSTRUCTION.\**

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 CLEAR sky in a place like London ; clearer and cleaner streets ; the emancipation in close places of one person from the empoisoned breaths and emanations of another ; room to breathe,—these are the blessings which the people are looking to their sanitary deliverers to give them.

If I were going to be one of the competitors for the munificent Westgarth prize on the purification of London, through the reconstruction of it, I should propose such a reconstruction as would be, in an architectural sense, perfectly revolutionary. I should propose to go upstairs to the tops of the houses to do it, and should indicate the construction of another London, overhead,—an upper London.

To convey what I mean, let us move to the best constructed, as well as the most beautiful street of this metropolis, if not of the world—Regent-street, in the part called the Crescent. That is laid out for

\* From an Address delivered before the Society of Arts on Wednesday, March 26th, 1884.

such a design as if it had been prepared for the experiment. All the houses are of the same height, and the height throughout is just right for a city like ours. It is sufficient to be handsome and commodious without being overwhelming, and without excluding the light from the streets. The roofs of Regent-street, at this part, are flat in comparison with other roofs. They are utilised here and there by the photographers for their studios, which, although temporary structures, stand firmly and well, in ready communication with the houses on which they are placed. The studio, where it exists, seems, naturally, to form and become a part of the house.

When we glance along the line of roofs, as on a level terrace, the idea of re-construction of all roofs, and of the re-adaptation of them, becomes very distinct and suggestive. The width of most London houses averages, as near as I can estimate, about 25 feet from front to rear. Here, then, is good space for a terrace for foot-passage. Imagine along two lines of long streets a terrace of this kind, with a handsome railing on each side, a perfectly level floor surface of wood, and, at intervals, light bridges spanning from one terrace to another, and you have an upper-day London which might almost relieve all the pressure from foot traffic in the streets below. Each house would have its own exit, or door, at the upper as well as at the lower parts; and, at convenient spaces, each terrace would be accessible from the street, as the Holborn-viaduct is at the present time.

It suggests, at first, a revolution of ideas to conceive such a change. It suggests much out of which a humorist can for a moment make capital. I know all this very well. But there is, in point of fact,



nothing more in it than in the first idea of making a tunnel under the streets or under a river. When the suggestion is looked at bit by bit, without prejudice, it offers more of sanitary advantage for the purification of the atmosphere, the protection of property, the comfort of the people in transit, the lodging of the people, the exercise of the young, and the beautifying of the whole city, than could be entertained on a mere general statement of the proposition.

In the first place, for every house in connection with an upper terrace, there would be the most perfect through and through ventilation of air. The staircase would no longer be a closed cupola for holding and storing all the emanations from the basement upwards.

In the second place, the fact of having terraces on the upper surface of London would lead to immediate arrangements for the purification of the air from smoke. So soon as the roofage was accessible as a terrace, the plan which Mr., now, Sir Spencer Wells projected for the removal of smoke from every house, by laying down horizontal conducting tubes with central exits and smoke-consuming furnaces, would be easily practicable, presuming always that some smokeless fire be not invented, or that coal gas do not become the fuel of the people. These terraces would then be the healthiest parts of London; charged with flowers and trailing evergreens, they would be the empyrean gardens of the great city.

The terraces, with their light intercommunicating cross-bridges in the long thoroughfares, would be more than pleasant footways and shady lanes for the foot travellers, or for travellers in light noiseless vehicles, like tricycles; they would be most useful for other purposes.



Along them the electric lines would pass and enter the houses direct ; and from them the letter carriers would most easily deliver their letters.

These terraces, while relieving the traffic in the streets below, would remove all necessity for the fire-engine, and would make London practically safe from fire. From them water would be supplied readily, a trained police for this upper London being ready at every moment to go down and extinguish fire in every domicile, carrying the hose with them, or plying it from above.

I think that no one who reflects will fail to see that all these changes would be advancements of great value for the health of a city like ours. They are, however, not the chiefest advantages. If any one will take the trouble to go, observingly, through the busy parts of London, where there are long miles of roadway—along Whitechapel and Mile-end, for example—he will see the most jagged, hideous lines of roofage. Here a line of houses two stories high ; there a row of three or four stories ; then a single house of five or six stories ; and so on, over and over again, like a set of bad teeth.

If the plan here suggested were carried out, all this would be rectified. A street like Regent-street expanded into a straight line would extend from the Marble-arch to the City, and from the City to the extreme East-end. The line of terrace pitched at five stories would necessitate the building up to the same level all the houses in that line, by which at least one-fourth more housage would be supplied, with arrangements for giving comfortable and healthy homes, beyond what now exist, to a fourth of the present population. The suspension cross-bridges would not be without their

compound service. They would be the bearers of electric lines along their side-ways, and would probably soon be utilised as centres from which electric beacons would be suspended to light the streets beneath.

Imagine the metropolis turned into a fairy land by this adventure of science into the domain of art, and art reciprocating the idea with all her rich resources, and we see in our mind's eye what our children, when we are all of us gone, may really see, and perhaps thank us for proposing for their benefit.

Objections may be made about mechanical and architectural difficulties. I heard them all made when the Holborn-viaduct was projected; I saw them all melt away as Colonel Hayward's practical mind came into work, and his unthanked skill, and industry, and responsibility, and genius, carried all before him.

It will be objected that flat-roofed houses are not weather-tight. In the year 1825, the then Parisian Asphalte Company roofed two houses with asphalte in Hinde-street, Manchester-square. I lived in one of those houses for twenty-eight years, and a better roof I never knew; but for the London smoke it would have been made into a garden. Men working upon it, walking over it, communicated no sound whatever into the rooms immediately below.

It will be objected that houses will not bear the weight of super-imposed suspended terraces for foot-walks. If they will not, they ought. In no direction would the sanitary improvement for the purification of a great city be more useful—as a side improvement—than in so reconstructing defective houses as to make them capable of bearing an equalised weight, which, carried by many, would, as we know from the bearing of ice, be comparatively light and practicable.

It is not to be disputed that in many instances dilapidated houses would have to be razed to the ground for the purpose of obtaining sound and safe foundations, and to those who maintain house properties in a condition requiring such an extreme measure, the idea suggested in this paper would, naturally enough, be objectionable. With the artist and sanitarian such an objection could not, however, carry much weight, because we are all agreed that for the health and beauty of the metropolis no reform is so much needed as that one which should be based on the demolition of old and dangerous tenements. In the course of such demolition and reconstruction, owners of property would themselves, ultimately, be amongst the most fortunate, as sharers in the improvements.

Of the many plans which have been suggested for giving space to crowded cities, such as terraces in the streets opposite to first-floor windows, tunnels subterranean, none seem to me to be half so practical, half so likely to secure the purification of atmosphere, as this, which I have now for the first time, after some years' hesitation, ventured to sketch out, not as expecting ever to see such a project realized in my own time, but foreseeing it as a necessity and a practicability in the times to come.

ANALYSIS OF ANIMAL STRUCTURES BY  
ELECTRICAL CONDUCTION AND RESIST-  
ANCE: WITH APPLICATIONS TO THE  
DIAGNOSIS OF DISEASE.

A RESUMED RESEARCH.

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**I**N the *Medical Times and Gazette* of 1869 I published three lectures on researches with the large induction coil then at work in the Royal Polytechnic Institution. In the second of those lectures, delivered on the 1st June in the year named, I introduced, in the last section, some attempts I had made for testing the conductibility of nerve, muscle, blood, and membrane, by a standard of electrical resistance.

The report I had then to submit did not come up to what I had hoped for. In the previous year, 1868, I had engaged, in pursuit of this inquiry, the invaluable assistance of the late Mr. Becker, of the firm of Elliott Brothers, an electrician who for mechanical skill and knowledge of electrical instruments was, in that day, unsurpassed. Mr. Becker, himself much interested in the research, was kind enough to fit up for me the most delicate and accurate of instruments then in use,

and made many excellent suggestions as to the mode in which to prepare the specimens for experiment.

The plan followed out was briefly this. Mr. Becker fitted for me a series of glass tubes of precisely the same length, one inch and a half, and of precisely the same diameter, one-sixth of an inch. Into each of these tubes at either end was inserted a copper rod of one-sixth of an inch diameter. The copper rod was passed into the glass tube one quarter of an inch, like a nicely fitting plug, with a little rim or collar to make the union complete.

In experiment, one of these little tubes was closed at one end by the insertion of the copper rod, and then the tube was filled up, to within half an inch, with the substance to be tested. Into the substance thus placed another copper rod, through which a fine hole had been drilled to allow for escape of air, was passed down so as to touch it. Lastly, the rods were fixed in their place with cement.

In this way a portion of animal structure one inch in length and a sixth of an inch in diameter was put in a position to form part of a circuit by attachment of the copper endings with the poles of the battery. The battery used was a small Daniell's cell, as the most constant, in Mr. Becker's opinion, at our command.

The mechanism was completed by putting the substance to be tested in a circuit connected with a galvanometer, invented, I believe, by Sir William Thomson, for refined measurements.

The results, as I have said, were not fully satisfactory. They were variable, even when the conditions under which the experiments were made were entirely the same. This variability we found to be due to decom-

position of the animal substance, a decomposition which, however feeble the battery, was sufficient to destroy the precision we desired to obtain. I nevertheless arrived at two results which did not vary.

(a) I found that blood conducted better than any other structure of the body; better than muscle, better than muscular fibre, better than white or grey nervous matter, better than membranous matter; better than water.

(b) I found that grey nervous matter conducted better than white.

I tried by multiplication of experiments to calculate the resistance from blood as a standard, but without avail. A general indication was all that could be obtained.

#### RESEARCHES WITH THE LARGE INDUCTION COIL IN 1869.

In 1869, in working with the large induction coil at the Polytechnic Institution, I returned to these inquiries by another mode of research, which succeeded, to some extent, well, and which had the advantage of enabling me to make the facts I had learned visible to large audiences.

The coil with which I experimented in these researches was the largest that had been made. It was constructed by Mr. Apps of the Strand. The coil weighed 1,500 pounds; its primary wire was 3,770 yards long; its secondary wire was 150 miles long; the central core of soft iron wire was 5 feet long, 4 inches in diameter, and 125 pounds in weight. The coil was worked by a battery of 48 Bunsen cells.

The spark given by this machine was twenty-nine inches long, and nearly a quarter inch in diameter. It



was always seen to be composed of two flames,—of a thin line of blue tense flame, and of a thick luminous whitish yellow flame, which could be blown away with the bellows from the central flame. As the spark flew across from point to point, it conveyed to the mind the idea that it would strike down any living thing in its way, but I found that beyond a slight scorching of very light combustible substances, such as fur or cotton wool, no effect was produced by it at all. Directed on a looking glass, it spluttered back, reflected in endless streams and with much noise, as if the glass were being shivered into fragments. This was, however, a mere noise, and wonderful sight; the glass was never injured in the least degree.

Directed on an animal body, or on part of an animal body, it was equally harmless. It would not kill, it would not even injure. All that occurred was, that if the shock were often repeated, a peculiar kind of gentle anæsthetic insensibility was produced. It did not even burn the skin.

In short, the spark, in this diffuse form, had no decomposing power. In order to give it intensity it was necessary to charge the Leyden jar from it and to discharge from the jar.

I took advantage of this diffuse spark to repeat the experiments on conduction of electrical discharges through animal fluids and tissues. The arrangement was carried out as follows :—

A tube one foot long and one-sixth of an inch in diameter was interposed in part of a circuit, consisting of copper wire one-sixth of an inch in diameter. The two ends of the wire, divided at the part where the tube was introduced, were inserted into the ends of the tube so as to form a connecting part of animal

structure within the tube, like a portion of one of the poles from the coil. The terminals of the coil were connected with two of Gassiot's vacuum cascade tubes. When the discharge was made from the coil, the vacuum cascades, if the conduction was good, were rendered beautifully luminous; while if the conduction was bad, the luminosity was reduced, or was wanting altogether.

*Preliminary Enquiries.*

1. I commenced my enquiries in this way by using a conductor of copper wire alone. The cascade was made splendidly luminous by the spark.

2. Into the circuit I introduced a tube prepared as stated above, and charged with freshly drawn defibrinated mixed arterial and venous blood. The cascade was still illuminated by the spark, but feebly, in comparison with what occurred in the last-named experiment.

3. I removed the tube containing the blood, and replaced it by a tube filled with animal fat. The spark would now fly from point to point outside the glass tube, but would not pass through the animal fat. No luminosity was shown in the cascade.

4. Another tube was introduced, in which distilled water took the place of the blood or the fat. There was some conduction and a faint luminosity in the cascade, but much inferior to that caused by blood.

5. A fourth tube was introduced, charged with the mixed grey and white matter of the spinal cord, freshly removed from a sheep recently killed. The conduction was very imperfect. There was the merest luminosity in the cascade, with frequent dispersion of sparks from point to point outside the tube.

*A Differential Enquiry.*

6. I made a differential demonstration, to show the difference of conduction between blood and nervous matter. I divided the positive pole of copper wire coming from the coil into two continuing wires, so as to be able to make a conductor from this pole communicate with each cascade independently, while the negative pole from the coil communicated with both. In the circuit, on the positive side, I interposed two different conductors,—one, a tube charged with blood; the other, a tube charged with nervous matter, mixed grey and white, of the spinal cord of a sheep that had been recently killed. By this arrangement, when the spark was discharged from the coil, the charge could take its course either through the blood or through the nervous matter, or through both, according to the facility afforded it, the result being shown in the cascades. The result was that the cascade connected with the blood was rendered beautifully luminous, while the cascade connected with the nervous matter was free of any illumination.

The inference I drew from these experiments was, that, in the body, the blood is the part which offers least resistance to the electrical current, either from the Leyden battery or from the secondary coil; and that a primary electrical discharge or a secondary current, would, preferentially, be conducted by the blood.

*Conduction by Blood under varying Conditions.*

From the experiments just related, I passed to others in which blood was subjected to the discharge from the coil, the conditions of the blood being variously modified. The fluid used in all cases was the fresh defibrinated blood of an ox recently killed.

1. I set up the apparatus in the same way as in the last-named of the vacuum experiments, that is to say, I used two vacuum cascades independently. The negative pole from the coil I connected with both cascades; the positive pole I connected by using two branches with each cascade. I was thus able to differentiate the conduction or resistance of different conductors. Next, in one branch from the positive pole I inserted a copper conductor for one cascade, and in the other branch a conductor of blood. On making circuit, the current passed preferentially into the cascade connected with the copper conductor, giving a brilliant light; none passed by the blood.

2. I removed the copper conductor, and replaced it by a conductor of leaden wire. The current, when turned on, passed, preferentially, through the lead conductor, instead of the blood.

3. Removing the lead conductor, I put in its place a conductor, enclosed in a glass tube, of defibrinated ox blood mixed with fifty per cent. of water. The current now passed, preferentially, through the undiluted blood column, which, in the two preceding experiments, had shown resistance in the presence of the metallic conductors. The cascade connected with the undiluted blood was illumined fully.

4. The diluted blood was lifted away, and in its place there was put a column of blood, condensed to one-half of its original weight by very gradual evaporation of its water. The blood, in this case, resembled an iron-coloured paste; and in this experiment, when the current was completed, the condensed blood took the conduction. The cascade in union with the natural defibrinated blood remained dark. The cascade in

union with the condensed blood was made beautifully luminous.

From these results I was led to the conclusion that the condensation and dilution of blood made a marked difference in respect to conductibility. Condensation favoured conduction ; dilution reduced it.

*Influence of Different Agents on the Conducting Quality of Blood.*

In continuance of the research with the large coil, I placed blood in the circuit under different conditions induced by the presence in it of different agents.

1. I placed a tube of fresh defibrinated blood in one of the branches of the circuit on the positive side. On the other branch I placed a similar column of blood, charged with five per cent. of chloroform, equally diffused, through it. On closing the circuit, the cascade connected with the natural blood was brightly illuminated. In the cascade connected with the blood containing the chloroform there was the faintest detectable luminosity.

2. I changed the blood containing chloroform for another column of blood saturated with pure anhydrous ethylic ether. The same negative result followed.

3. I changed the column of blood charged with ether for another column of blood holding one five hundredth part of nicotine. The same negative result followed.

4. I made another modification. In place of blood holding nicotine, I interposed some in which the blood was charged with ten per cent. of ethylic alcohol of sp. gr. 0.795. The alcohol had been added to the blood so gradually that no coagulation from its presence was perceptible. The result was a conduction not materially



different from that which was observed in the natural blood. If more alcohol were added, the conduction was interrupted.

5. I once more made a change. In place of the column of blood containing alcohol, I inserted one holding a five hundredth part of strychnia in solution. In this experiment the conduction, while it occurred through both conductors,—natural blood and strychninized blood,—seemed most perfect through the latter. The result was less decisive, however, than in previous cases.

In these experiments with blood, the tubes containing the blood were twenty inches long and one-sixth of an inch in diameter. The tubes were closed at their ends with copper rod, which came into contact with the structure enclosed at its extremities, and the copper was fixed firmly with gutta-percha cement. The tubes were of glass, and were each of the same bore, size, and weight.

#### RESEARCHES WITH THE CONDENSED SPARK-SHOCK, 1869.

It may be objected that the researches above related were all made on dead animal matter. Some further researches had reference to the passage of electrical currents of high tension through living bodies.

It occurred to me that animals about to be killed might, in an instant, be struck dead by an artificial lightning stroke, without the infliction of the slightest pain.

To put this to the test I set up a Leyden battery, presenting forty square feet of surface, and charged it by three full discharges from the coil.

The jars of the Leyden battery were set in what is called cascade. They were set six in a line, each care-



fully insulated by means of a vulcanite plate. The outside metallic plate of jar No. 1 was made to communicate with the inner metallic plate of the next jar, and so on through the series. The inside of the first jar was finally connected with the positive, the outside of the last jar with the negative pole of the coil.

The discharge from this battery was one of intensity. It gave no diffuse flame, and it did not pass over surfaces of the body, but pierced them, and, striking a living animal body, stunned or killed it instantly. By this shock a pigeon was killed so instantaneously that it gave no sign of having been struck at all. It did not move from its perch, but remained there, hardened in all its tissues as if it had been transformed into wood, like a part of the solid material which supported it.

On larger animals the shock was destructive of life, and with equal rapidity, but the system of the animal was less generally affected. With the view of killing without the infliction of pain, I had sheep that were about to be slaughtered brought to the coil,—on one occasion six at one time,—and had them killed by this artificial lightning. On inspection of the bodies of the animals so killed, the facts elicited were :—

(a) The structure of the brain and spinal cord showed no sign of injury, no mark, no rupture.

(b) No sign of injury was found in the lungs.

(c) No sign of injury was found in the liver.

(d) No injury appeared in the voluntary muscles.

(e) In the circulatory system, wherever there was traverse of blood, there was injury.

(f) In the alimentary canal there was always evidence of the course of the current, and of injury from it.

It seemed clear that in every one of these observations, forty in number, the blood was invariably traversed

by the current, but that sometimes the alimentary canal shared in the conduction. The heart usually was engorged with blood on the right and in many instances on the left side. In one case the right auricle was ruptured by the distension of blood. The vascular membranes were congested, and the sinuses of the brain were extremely distended.

Through the alimentary system there was in every case much vascular congestion of the mucous surface. In one instance the stomach was ruptured. In another instance the whole tract of the small intestines was injected to dark purple.

In one case only did the nervous system appear to be affected. In that instance there could be no mistake that the sympathetic nervous chain had been a conductor of the shock. The whole of the visceral ganglia and connecting nervous lines were marked out by a dark, I may say black, discoloration, as if they had been tinted with black ink. The phenomenon was so striking that the butcher, who had conveyed away the carcase of the animal, a sheep, to dress it, came to fetch me to see what had occurred. In this instance the alimentary canal had conducted the discharge largely, but the blood was, at the same time, as much and as characteristically affected as in other examples.

On the whole, from the phenomena observed, I came to the conclusion that the cause of the sudden death was the direct effect of the electrical discharge upon the blood, and the instantaneous expansion of the gaseous part or atmosphere of the blood.

The observations led me also to the same conclusion as I had arrived at from the previous researches, namely, that the blood is the prime conductor of any electrical current through the body, and is selected preferentially

to any other. The alimentary canal is probably only so good a conductor because of its rich vascular supply.

#### RESEARCHES SINCE 1879. ANALYSIS OF TISSUES BY THE ELECTRIC BALANCE.

In 1879, Professor Hughes, F.R.S., the distinguished discoverer of the microphone, introduced his electric balance, and was so good as to show it to me in an early stage. He gave me every opportunity of using the balance for physiological and medical purposes, and on May 15th, 1879, I communicated to the Royal Society a report of some enquiries I had carried out with it in cases of deafness. For that practical work the balance was most useful. It is equally good in testing the conduction and resistance of animal structures, for which I have since employed it.

To those who are not well acquainted with the balance, the following brief account of it may be necessary before any narrative of research with it is related.

In working with the balance, the determinations of results are taken by the ear, that is to say, by the sounds heard through a telephone, moderated by the conduction or resistance of the connecting medium.

The instrument, as it has been used for the present purpose, consists of a Leclanché's cell for the battery, a microphonic key connected with the cells and with two fixed primary coils, and a secondary or induction coil the terminals of which are attached to a telephone. The induction coil moves on a bar between the two fixed coils, and the bar is graduated into 200 parts, by which the readings of sound are taken. The graduated scale is divided into 20 centims., and each of these divisions is subdivided into 10, so that the sound may be tested from the maximum of 200 units to 0°—zero. The fixed

coil on the right hand contains 60 metres of wire; the fixed coil on the left hand contains 100 metres. By this means a long scale from the left hand coil is produced. The secondary coil contains 100 metres of wire.

The Leclanché cell was selected originally by Professor Hughes as affording a reliable current for standard comparisons. In using the instrument, the induction coil is moved along the scale from or towards the larger primary as may be required, and the degrees or units of sound are read from the figures on the scale, the sound being made by the movement of the microphonic key between the battery and the primary coils.

In observing with the balance, every precaution has been taken to secure systematic correctness and character. In all cases many readings have been made before the final determination has been recorded. The room in which the research has been carried out has been free of noise, and the time of observation has been the night, when everything was stillest.

#### MODE OF PROCEDURE.

I have preferred to calculate in these researches from conduction as well as resistance. I use this preference because I have felt it necessary to employ a substance belonging to the animal body as a standard from which to move and by which to compare, instead of taking some substance like copper wire, which is foreign to the animal body.

As water forms more than 60 per cent. of the animal body, and as it is already the standard for specific gravities of animal structures, I have selected it as the standard for measuring the electrical conduction and resistance of the animal structures.

I have also taken 98° F. as the temperature at the

time of observation, that being, within two-fifths of a degree, the natural temperature of the human body.

By observation with the electric balance which I possess, I found that when every care was taken to hear with the greatest precision, and when, with the ordinary metallic conductor, the observer could hear sounds from the telephone from 200 units down to zero, the interposition into the circuit of a column of distilled water, as described below, at a temperature of 98° F., caused the sound to be lost at 60 units from the maximum, or 140 units from zero. With water, therefore, at 98° F. as the standard, conduction is represented by 60, resistance by 140.

#### DETAILS OF RESEARCH.

In order to make the comparisons in research as accurate as possible, I constructed a number of small gutta-percha troughs, each one an inch and a half long, one-quarter inch broad, and one-quarter inch deep. Into the ends of these troughs a copper wire, one-sixteenth of an inch in diameter, was pushed, until the terminal points of the wire in the trough were an inch apart. Into this trough the water or other substance to be tested was placed, so as to come into the most perfect contact with the copper wires, which, by their free ends, were connected with the terminals from the telephone. The troughs were firmly supported on a strong wooden stand, and, as a rule, the same trough was used in each set of experiments after it had been carefully cleaned and dried. By this means each substance experimented on was brought into the same condition for experiment without disturbance of the terminals within the trough.



*Observations on Blood at 98° F., by Water as the Standard.  
Conduction 60 units; Resistance 140.*

|                                                                               | CONDUCTION<br>UNITS. | RESISTANCE<br>UNITS. |
|-------------------------------------------------------------------------------|----------------------|----------------------|
| 1. Defibrinated fresh blood . . . . .                                         | 180                  | 20                   |
| 2. Clear serum of blood . . . . .                                             | 175                  | 25                   |
| 3. Clear white fibrine . . . . .                                              | 53                   | 147                  |
| 4. Fresh blood-clot . . . . .                                                 | 183                  | 17                   |
| 5. Blood-clot condensed, one-third by evapo-<br>ration of its water . . . . . | 195                  | 5                    |
| 6. Blood-clot condensed one-half . . . . .                                    | 158                  | 42                   |
| 7. Defibrinated blood, condensed one-fourth<br>by evaporation . . . . .       | 180                  | 20                   |
| 8. Defibrinated blood, condensed one-half . . . . .                           | 185                  | 15                   |

The blood and the serum tested in these experiments was obtained from sheep's blood immediately after the death of the animal at the slaughter-house. The sp. gr. of the defibrinated blood, at 60° F., was 1052; of the serum, 1028.

*Observations on Nervous Matter. Temperature 98° F.*

|                                                                                                 | CONDUCTION<br>UNITS. | RESISTANCE<br>UNITS. |
|-------------------------------------------------------------------------------------------------|----------------------|----------------------|
| 1. White matter from medulla oblongata, care-<br>fully separated . . . . .                      | 117                  | 83                   |
| 2. Grey matter from cerebral convolutions . . . . .                                             | 130                  | 70                   |
| 3. Portion of spinal cord, with the <i>pia mater</i><br>surrounding it . . . . .                | 137                  | 63                   |
| 4. Brain substance, white and grey, after diges-<br>tion in ether, for removal of fat . . . . . | 135                  | 65                   |

In the above analyses the fresh brain of the sheep was used, at a temperature of 98° F.

*Observations on other Tissues and Structures.*

|                                                           | CONDUCTION<br>UNITS. | RESISTANCE<br>UNITS. |
|-----------------------------------------------------------|----------------------|----------------------|
| 1. Muscular structure of tongue, fresh . . . . .          | 115                  | 85                   |
| 2. Bone freed of periosteum . . . . .                     | 000                  | 200                  |
| 3. Tendon, cut from centre . . . . .                      | 130                  | 70                   |
| 4. Periosteum from fresh bone . . . . .                   | 70                   | 130                  |
| 5. Cellular tissue from betwixt skin and muscle . . . . . | 125                  | 75                   |
| 6. Under surface of skin . . . . .                        | 165                  | 35                   |
| 7. Fresh dura mater . . . . .                             | 50                   | 150                  |
| 8. Pure fatty tissue . . . . .                            | 000                  | 200                  |
| 9. Membrane of fat . . . . .                              | 000                  | 200                  |
| 10. Fresh marrow . . . . .                                | 70                   | 130                  |
| 11. Albumen of white of egg uncoagulated . . . . .        | 165                  | 35                   |
| 12. The same albumen coagulated . . . . .                 | 165                  | 35                   |



|                                                                                 | CONDUCTION<br>UNITS. | RESISTANCE<br>UNITS. |
|---------------------------------------------------------------------------------|----------------------|----------------------|
| Pure gelatine, dry . . . . .                                                    | 000                  | 200                  |
| Pure gelatine dissolved in twice its own weight<br>of distilled water . . . . . | 160                  | 40                   |
| The same after solidification . . . . .                                         | 145                  | 55                   |
| Fresh pus, sp. gr. 1030 . . . . .                                               | 140                  | 60                   |

The observations here tabulated are fair averages of conduction and resistance in the tissues named.

The research has been modified in various ways, as by testing admixtures of water and blood; blood treated with different metallic salts, and with medicinal agents; albumen diluted with fat; gelatine admixed with calcium phosphate; blood treated with excess of saline chlorides; blood made chylous with fat. Some pathological structures have also been tested. These details must remain for another paper.

### *Deductions and Inferences.*

For the present I am content to indicate from what has been recorded two sets of deductions and inferences—one physiological, the other diagnostic.

1. The first physiological inference is, that if in the living animal body there be electrical conduction, such conduction must be by the blood. I cannot conceive how nervous matter, with its high resistance, can conduct in the presence of blood. If in life it does so, it must be in a condition quite distinct from what it is in death. From the chemical composition of it, that view is hardly tenable, and I should infer, using the language of the first electricians, that, to the blood as a conductor, the nervous matter would be an electric and insulator.

Another physiological deduction which I gather is that each tissue admits of being analysed and detected by the electrical balance, and that even mixed tissues

may be determined as to their composition by this means. I can already, by the few elementary rules I have acquired, determine, from a mixture of albumen, fat, and sodium chloride, how much of each enters into the composition of the mass. I can detect in a specimen of blood the changes incident to its dilution with distilled water, the removal of water from it by evaporation, or the addition to it of soluble saline substance, like sodium chloride.

I infer that the parts which conduct are the water, the salts, and, perhaps, the red corpuscular matter of the blood. The pure colloids, judging from the behaviour of fibrine and gelatine, conduct only by virtue of the water they carry. Hydrated sufficiently, they conduct precisely as water does, and, as they lose water, they offer resistance. This was well marked in the experiments with blood-clot, where the water, the fibrine, and the corpuscular matter alone remain; while in the defibrinated blood the removal of water increased the conduction, owing, no doubt, to the concentration of saline, and, perhaps, of corpuscular material.

Subject to correction on further enquiry, I think that the physical change called coagulation increases conduction in blood. I observed also that certain increases of temperature favour conduction.

2. The diagnostic inferences I glean from these observations are that the study of electrical conduction and resistance must, in course of time, become one of the most important aids to diagnosis. Take for an instance a tumour, the nature of which is obscure. If it be composed of cellular elements, it will have a fair conduction. If it be a pure fatty tumour, it will not conduct at all; the resistance will be 200 or

the maximum resistance. Take a fluctuating tumour. If it be a cyst containing serum, the conduction from the needles touching the contents will be high, say 175 units. If it contain fat, or any fluid holding fat, the conduction will be lower or absent.

As between a fibroid uterus and an encysted fluid growth, there would, I think, of necessity, be a difference of conduction which the balance would analyse. As between medullary cancer and scirrhus, there would, I suspect, also be a marked difference. As between a part of the body containing a metallic substance like lead or mercury, and a similar part containing none, there would no doubt be, as in Professor Hughes' startling experiments with metals and alloys, a distinct difference of conduction. These and numerous other diagnostic values, admitting of being determined by the method described, will occur to every practitioner. I shall, of course, return to the subject again, but meanwhile I have felt it necessary, that time may not be lost, to give an exposition of what I have so far accomplished, in order to set others to work in continuation and in perfection of the design.

*AN ESCULAPIAN POET—JOHN KEATS.*

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**I**N the years 1813-14, when Mr. Astley Cooper was rising towards his zenith, and when Bright was a medical neophyte fresh from Cambridge, there lived in the Borough as students of Guy's and St. Thomas's, three young men, who occupied the same rooms, and who seem to have enjoyed their lives together as only men in the spring of life can. Their names were John Keats, Henry Stephens, and George Wilson Mackereth. The two last-named lived on to a ripe age; the first died in his twenty-sixth year,—died at Rome on February 3rd, 1821, of consumption, under circumstances of some romance, as the world fancies. A poet he,—John Keats the poet,—the man who, by writing one immortal line, placed himself for ever before his kindred speaking his tongue, even though they knew nothing else about him, his life, his work, his death. The two companions of the poet, whose names I have just written, Stephens and Mackereth, were both known to me. Mr. Stephens I knew well, knew as one of my dearest friends for eight years, namely, from 1856 to his death in 1864. Mr. Mackereth, whose daughter Agnes married Mr. Stephens' eldest son,

the present Mr. Henry Stephens, of Avenue House, Finchley, I also knew, but less intimately.

My dear friend Stephens was never weary in telling me about Keats. The two students had allied sympathies. Both were pursuing medicine when they lived together, and both left it, after entering it, to follow other pursuits. Both were touched with poetic fire, and from a drama, *Edwy and Elgiva*, written by Stephens in his early days, I expect that had he not been led into other and more remunerative lines, he, too, would have ranked as a poet. He had imagination, original observation, industry. In medicine he was proficient. We are indebted to him for an excellent work on *Hernia*, and for the introduction of creosote into medical practice as a therapeutical agent.

John Keats is our man now, on whose life we will for a moment refresh our memories, and whose short life's labour we will contemplate; happily a sweet contemplation, though planted in much sorrow; a paradise of beauty in a wilderness of despair.

It is fortunate for this task that the basis on which it may be laid is good. With truly loving and artistic hand, with conscience, with prescience as to the future of his author, Mr. H. Buxton Forman, himself of medical parentage, has in four handsome volumes brought together all that is up to this time known reliably of our Esculapian poet, his life, letters, sayings, poems, with commentaries and elucidations, which bring the youth straight to the reader. Such mastery of details with concealment of ungrudging toil is here combined, that when the volumes have been carefully read, it is as if, after infinite wanderings above, below, around, in search of the poet, the biographer had literally found the man, and brought him personally to introduce him



and his greatness. You almost put out your hand, hoping to grasp that of Keats in life before you, and ask him, "How are you, old friend?" as you rise from the books after closing up with the delicate and perfect preface which supplies the biographer's motive, and indicates his accomplished duty.

To get at the poet through these handsome volumes it is readiest to begin with the last volume, and read that first. In the last volume there is an appendix, in which Leigh Hunt, Benjamin Haydon the painter, Cowden Clarke, George Keats, and Joseph Severn who was with Keats at his death, with others of less knowledge of the man, give their histories of him. The records thus collected and gathered from such various channels are remarkable in that they generally agree in analysis of character and personnel. They agree, too, I may add, most closely with the views respecting the poet which Mr. Stephens conveyed to me between 1856 and 1864.

The portrait presented of Keats on the plate opposite, kindly lent to me by Mr. Buxton Forman, in whose possession is the original, brings the poet before us in his twenty-third year. The face tells its own story, all that can be read from physiognomy. With this it should be recorded that the stature of the man was small, and the body, although well-shaped, delicate. Mr. Stephens remembered him as being called by his fellow students "little Keats," he being, at his full growth, no more than five feet high. In the lecture-room he seemed to sit apart,—I am retailing Mr. Stephens' recollections,—and to be absorbed in something else, as if the subject suggested thoughts to him which were not practically connected with it. He was often in the subject and out of it, in a dreamy way.





Yours affectionately  
John Reats.



He never attached much consequence to his own studies in medicine, and, indeed, looked upon the medical career as the career by which to live in a work-a-day world, without being certain that he could keep up the strain of it. He nevertheless had a consciousness of his own powers, and even of his own greatness, though it might never be recognized,—an idea which he maintained, though it may seem a paradox, to that almost last moment of his frail physical existence when he dictated his own epitaph,

“Here lies one whose name was writ in water,”

as if, knowing his earthly immortality, he doubted that he had impressed it on the world. Amongst his fellow students Keats was respected, and, by a few, loved. Leigh Hunt's after description of him as “a mighty soul in a little body” was understood long before by his medical contemporaries. Not only this; the little body was filled with courage, too, or, vulgarly speaking, pluck, which showed itself once in a fair fight with a big butcher lad, who, lubberly and faint-hearted, got a thrashing from the small poet which he would not soon forget.

To the dreamy, courageous, gentle, mighty spirit in the little body many things were at times sore to bear. The love of beauty inborn, and which made this poet of the beautiful what he was, was not all satisfying. Like Akenside, he was touchy on the subject of his parental origin. He need not have been. His father, who was killed by a fall from his horse when the poet was a school-boy, was a natural-born gentleman, handsome in feature and figure, honourable in all his dealings with the world, wise, prudent, and so much liked that his visit to the school at Enfield, where

his boys were educated,—school kept by the eminent and excellent John Clarke,—seems always to have been looked on as an event of pleasure. His mother, who also died young, of consumption following upon rheumatism, was a handsome, an intelligent, and a good mother. And the poet loved them both with all his heart. But the father was, unfortunately, a livery stable-keeper under Mr. Jennings of the “Swan and Hoop,” on the pavement in Moorfields, opposite the entrance into Finsbury Circus, and the mother, Miss Jennings, daughter of the proprietor. After the death of Jennings, Keats senior continued the proprietorship of the stables, and there his wife gave birth to all his children.

In the medical schools this origin of the student poet was well known, and though doubtless it was as good as that of many others there, and better than some, it seemed, as Keats too sensitively thought, to use Mr. Stephens’ words, “to stick to him.” Later on in life it still evidently bit, and made him touchy even towards Shelley, as towards one of better blood.

Returning to the last volume of the Forman edition, and rapidly reviewing the life presented there, we find that Keats was born at the address already given on October 31st, 1795, the eldest of a family of four, consisting of himself, John, brother George, brother Thomas, and sister Fanny who is still alive. In 1803, one year before the death of his father, and four years before the death of his mother, he goes to Mr. John Clarke’s famous Enfield school, remains at school there until 1809, and then, apparently not by his own choice, but by the will of his guardian, Mr. Abbey, is articulated to Mr. Thomas Hammond, a surgeon practising at Church Street, Edmonton, two miles from Enfield. His terms of apprenticeship would

be for five years, but perhaps were not carried out to completeness, for he and Hammond quarrel, and he, it may be gathered from his own words, even shows fight with his master, and leaves him rather abruptly. He has touched poetry by this time, has translated the *Æneid*, written in imitation of Spenser, and given wings to imagination. Leaving Hammond, but not leaving physic, he enters, in 1813-14, as a student to the then combined school of Guy's and St. Thomas', and continuing to develop poetry there, writes aptly enough an ode and hymn to the god of physic and of song, Apollo. In 1815-16 he is the friend of Leigh Hunt, editor of the *Examiner*, and publishes his first poem in that newspaper. In 1816 he is introduced to Haydon the painter, and becomes the intimate of Charles Wentworth Dilke, Severn, and other celebrities. In 1817 he publishes his first volume of poems, dedicated to Hunt, and in the same year, during part of which he resides at the Isle of Wight, Margate, and Leatherhead, he commences his poem of *Endymion*. In the autumn of the same year, 1817, he visits a friend, Bailey by name, at Oxford, and in that visit runs loose, and pays a forfeit for his indiscretion, which ever afterwards physically and morally embarrasses him. In the winter of 1817-18, he takes up residence at Hampstead, and after remaining there until March 1818, joins his brothers at Teignmouth, Devon, where Thomas lies fatally stricken of consumption. In April of this year *Endymion* sees the light, and in the same year *Blackwood* and the *Quarterly* subject him to the penalty of critical fire.

In this same eventful year—1818—Keats finishes a new poem, *Isabella, or the Pot of Basil*; bids farewell to his brother George and brother George's new wife on their departure for America; visits the Lakes in company



with a friend, Charles Armitage Brown ; makes a tour to Scotland with the same companion, and gets over to Ireland, during which tour he contracts, in the island of Mull, what is to be a fatal cold. He returns to Hampstead in August, visits Teignmouth in September, and again to Hampstead for the winter, first at Well Walk, and afterwards, with Armitage Brown, at a house in Wentworth Place, Hampstead, now called Lawn Bank, where the late eminent physiologist, Professor Sharpey, as many well remember, passed some years of his life in his declining days.

In the winter of this eventful 1818, two other events are memorable in the life of our poet. The first of these events is the commencement of his greatest poem, *Hyperion* ; the second, to him, is more eventful still. Hitherto he, with a lively fancy, has laughed at men in love ; never could see any so truly simple as men in love—fools all. Now it is his own turn to succumb to the temptation, and become, as he, in other tone, would have said, the greatest simpleton of all.

The lady who conquered the obdurate, dreamy, cynical poet, lived with her mother in a house next door to the house at Wentworth Place, where the poet lived with Armitage Brown. The name of the lady was Fanny Brawne. She and her mother were friends and patients of another friend of mine, and one whom, in his retirement from medical life, the profession generally still remembers with much affection and respect, Mr. C. J. F. Lord.

All the loving romance of Keats' heart fastened on this young lady. His letters to her, ecstatic, tender, biographical, childish, jealous, mournful, form a considerable part of the last of the Forman volumes. His love remains to the last a sad picture. When he is far



away in Rome, dying of consumption, three letters of hers, which he has not the strength to read, are put aside by his faithful friend, Severn, to be laid over his heart in his coffin when he is dead, and buried with him.

Fanny Brawne, appearing in the life of Keats, is well carried by her name,—respectable, commonplace, hardly beautiful, if the picture we have of her be honest, but prim, lively, and not very sentimental. A flirt, perchance, as poor Keats felt, from what he observed in her behaviour with his friend Brown, but harmless withal; certainly none of the glorious creature which the poet made her. Their engagement, if so it may be called, came to nought. The poet's death sealed the fate of that, and left the lady to marry a less romantic lover than her first, whose letters, nevertheless, she held unto her life's end.

In the year 1819, after spending some time at Winchester and Bedhampton, and writing the exquisite poem, *The Eve of St. Agnes*, Keats comes back to Hampstead with indications of declining health; has persistent sore throat, and, as his letters indicate, is very sad, repentant of some things, irresolute in others, and hopeful in few, if any. In the summer time he visits the Isle of Wight, and later on Chichester. At various intervals his poetic labours progress. He composes the *Ode to a Nightingale*, and *Lamia*; gives up *Hyperion*, on which he was at work, except the published fragment of it, which carries us into the vestibule of its projected glories, and leaves us there, bewildered and half blinded, as gods amongst gods. Then, with Brown, who evidently sways the poet by a strong will, he writes *Otho*, a tragedy, Brown supplying the bricks and mortar, the details of the plot and Keats doing the architect's work, constructing the

whole, and giving whatever grace and beauty belongs to a work so unspontaneously bidden.

On the whole, the year 1819 ends with our poet unhappily. At its close he is once more at Hampstead, with his mind at sea. He will follow literature as a profession; no! he will go back to his apothecary work; no! he will travel. He is already very ill, worse than he himself knows, throat still sore, and mind and body agitated and feeble. In this period, as we find from a letter to his sister, he leaves off animal food, that his "brains may never, henceforth, be in a greater mist than is theirs by nature."

The close of the year 1819 leaves Keats still constitutionally ill; and in the early part of 1820, first days of February, his fatal illness commences. He reads his own doom with the first spitting of blood. "It is arterial blood," is the comment he makes, and from that time he has really no hope, though promises of recovery seem now and then to offer. His poems, *Ode to a Grecian Urn*, *Lamia*, and *Isabella*, are published, but he, now an acknowledged poet, is losing vital power. He moves from Hampstead to Kentish Town, to be near to Leigh Hunt, and for a time lives with the Hunts. Here his illness increases, and, in the opinion of a very able and advanced physician, Dr. Lamb, one of the first pioneers of the vegetarian movement, he is "under sentence of death."

Returning to Wentworth Place in August, to be nursed by Mrs. Brawne and the beloved Fanny, he remains there a month, and then determines to leave, with his friend Joseph Severn, for Italy, to winter there. He departs in September, and on the voyage writes the sonnet, beginning,

"Bright star, would I were steadfast as thou art,"

his last strain. The two friends settle finally in Rome.

The description of the close of the life of Keats in Rome, written by Severn, is most touching. The simplicity of affection of the writer, who died but the other day, makes it the most beautiful of narratives. Of all the friends who noticed Keats, from genial Hunt to sublime Shelley; of all the hands that helped him, not excluding George Keats, his brother, and the Brawnes, no friend was the like of Severn. It is a real happiness to know that the world does sometimes, by some good fortune, possess such a nature of love and constancy.

In Rome, Keats came under the care of Dr., afterwards the well-known Sir James, Clark, the friend and physician of Queen Victoria. From Clark all that could be effected by the medical skill of that day was rendered, together with much generous kindness from his lady. But medicine and medical art were unavailing, and on February 23rd, 1821, in early morning, the words, "Severn—I—lift me up, I am dying. I shall die easy; don't be frightened; be firm, and thank God it has come," are the closing utterances.

Three days later they bury him near the famous tomb of Cestius; and not long after the ashes of Shelley, to whom he was the Adonais, rest by his side.

Who would follow out further the details of the life of this remarkable youth, may seek them with confidence in the work by Mr. Forman. Let me touch for a moment on one or two characteristics of the poet, and so conclude.

John Keats was like a meteor, which, wandering in the eternal spaces, and coming into contact with, and friction with, the atmosphere of a world, bursts

into flame, and dazzling the beholders on the world with its brightness, suddenly dies out. How so fragile a life chanced to be recorded at all as a wonder is, in itself, as great a wonder. The idea suggested by Byron that he was "snuffed out by an article" in *Blackwood* or the *Quarterly Review* is simply nonsense. His death, expedited, perhaps excited by his own imprudence, was due to a disease which was latent in his nature, hereditarily stamped there. The reviews, bitter, intense, selfish, the work, as all such works are, of unwholesome minds, did, in fact, but serve, by the friction they caused, to bring Keats into permanent notice. But for them, Hunt, Haydon, and Hazlitt would not have been so loud in commendation; Shelley would not have written the immortal *Adonais*, Byron the famous stanzas. But for them, Severn would perhaps not have been so tender, so persistently watchful, so determinately tuned to the idea that, *nolens volens*, the memory of his friend should never die. We may throw aside sentiment anent Keats altogether in so far as reviewing goes. There has been on this earth many a Keats, who, failing to undergo such friction, has died unknown.

To be so far honest to the memory of Keats is not to lessen nor to loosen his fame as a poet. A poet true and exquisite he was, "of imagination all compact." "Byron describes from observation, I from imagination," was his own exposition of himself, and it was sound. He was a man who dreamed rather than existed in this world. He lived, as it were, in some vast observatory, looking out on mighty nature. In a room, Mr. Stephens told me, he was always at the window peering into space, so that the window-seat was spoken of by his comrades as "Keats' place." Here

his inspiration seemed to come most freely. Here, one evening in the twilight, the two students sitting together, Stephens at his medical studies, Keats at his dreaming, Keats broke out to Stephens that he had composed a new line :—

“ ‘ A thing of beauty is a constant joy.’ ”

“ What think you of that, Stephens ? ”

“ It has the true ring, but is wanting in some way,” replies the latter, as he dips once more into his medical studies.

An interval of silence, and again the poet :—

“ ‘ A thing of beauty is a joy for ever.’ ”

“ What think you of that, Stephens ? ”

“ That it will live for ever.”

The touch of student life and manner here told, as Mr. Stephens told it me, brings out our poet in his natural form and character. A line, one line !—a thought, one thought in a line, and the fragile becomes the imperishable poet.

In his poetry, it is by catches of immortal thoughts of this kind that Keats still lives. The reviewer was quite right who said it was all but impossible to read through *Endymion*. The opening line above quoted saves it, makes it, and in every poem, except one, some line or expression is the point of inextinguishable fire.

The exception is *Hyperion*, a fragment so called, published by the publisher's will, not the poet's, opening in splendour, and ending abruptly in a dream so marvellous that the reader who truly reads it sinks at finding the dreamer beginning to awake in the dawning of his conception. A chasm infinite :

“ All the sad spaces of oblivion : ”



the gods of the old earth in darkness visible: Saturn prostrate: the mother of the gods, "in whose face was no joy:" in the midst of the gloom the sudden light from Hyperion on a granite peak: the voices of the Titans calling to him to let him know Saturn is with them: Saturn! Saturn! Saturn! To read is to see, to hear, the gods themselves; and though the unfinished short concluding part of the poem weakens the effect, there is nothing in Milton more sublime.

From the sublime to the humorous, if not to the ridiculous, is a steep step in the case of Keats, and yet is one that may be taken. In Keats there was a fund of humour, which was broad at times, even to mischief and practical joke. His friend, C. Armitage Brown, let a house of his to a Jewish gentleman named Benjamin. The water which supplied this house was in a tank "sided with lime." One day Brown, on going from home for a short time, said to the poet, "Keats, if any letters come for me, do not forward them, but open them, and give me the marrow of them in a few words." At the time when Keats, a little later on, wrote to Brown, no letters had come, so he thought he would invent one, and "dabbed off a short one, which was the reason of the joke, exceeding his expectations." Taking advantage of the lime and the water cistern, he pretended that Mr. Benjamin had written to Brown the following short note:—

"SIR,—By drinking your d——d water I have got the gravel. What reparation can you make to me and my family?—NATHAN BENJAMIN."

"By a fortunate hit, I hit," says Keats, who tells the story himself, "upon his right heathen name—his right prenomen. Brown in consequence wrote to the surprised Benjamin the following:—



"SIR,—I cannot offer you any remuneration until your gravel shall have formed itself into a stone, when I will cut you with pleasure.—C. BROWN."

Mr. Benjamin answered, insisting on an explanation, and then, of course, the explanation in full was discovered, to the amusement of all concerned. Mr. Snook, a mutual friend of Brown and Keats, nearly burst "the hoops of his fat sides."

In another place he has a humorous tilt at the bicycle of his day:—"The nothing of the day—1819—is a machine called a 'Velocipede.' It is a wheel-carriage to ride cock-horse upon, sitting astride and pushing it along with the toes, a rudder wheel in hand. They will go seven miles an hour. A handsome gelding will come to eight guineas; however, they will soon be cheaper, unless the army takes to them."

From sublime poetry and humour to practical wisdom, which might be called proverbial, is another steep step, and yet is there in the life of Keats. In perusing his many letters, written all in simple love and friendship, with no suspicion that the world would ever see them, we drop constantly on short pithy sayings, which it would be well worth while to string together for ready reference. I will add a few:—

"A man of great executing powers at twenty, with a look and speech almost stupid, is sure to do something."

"Poetry should be great and unobtrusive, a thing which enters into one's soul, and does not startle it or amaze it with itself, but with its subject."

"It is impossible to live in a country which is continually under hatches."

"There is a continual courtesy between the heavens and the earth."

“A mighty Providence subdues the mightiest minds to the service of the time.”

“Circumstances are like clouds, continually gathering and bursting.”

“Nothing ever becomes real till it is experienced ; even a proverb is no proverb till life has illustrated it.”

“Every point of thought is the centre of an intellectual world.”

“The genius of poetry must work out its own salvation in a man. It cannot be matured by law and precept, but by sensation and watchfulness in itself.”

“That which is creative must create itself.”

I have taken these sentences as they have occurred, without special selection. The letters are full of such short and terse sayings, showing an imagination checked and controlled by a reason which was daily ripening into fruit of choicest wisdom. Even the poetry, in the later stages of it, when the “slipshod *Endymion*” had shown its own faults to the inventor of them, was becoming more and more chastened and reasoning, except when the wild flame of love checkmated the spirit of wisdom.

Of the poetry itself I add no illustrations. The reader who takes up the Forman volumes, in which are included the admirable “remains” which Mr. Monckton Milnes (Lord Houghton) collected with so much judgment, taste, and feeling in 1848, will read all the poems, and love them as they are deserved. He will find the poetry, fanciful often as to form, based really on three masters, Spenser, Shakespeare, and Milton ;—the descriptive Spenserian in tone ; the wise and dramatic, Shakespearian ; the epic, Miltonian. Of his own time and spirit, Keats drew no breath as a poet. In that bad time

“His soul was like a star, and dwelt apart.”

The pleasures of men who mistook biting wit for humour, and tinkling sentiment for poetry, were not of his ideal. For the men of real genius of his time, Wordsworth, Shelley, Byron, Haydon, he was not stingy in his admiration; least of all was he jealous of their fame; but he stood apart from them also as masters from whom to copy or to learn.

It has been said that Keats, although educated for our own profession of medicine, had no talent for it. In that vulgar acceptance of the term, which converts talent into success as tested by the money-getting standard, the height and splendour of the idol of the market-place, that may be true. It requires an immense amount of inborn stupidity, and withering of heart, and lowering of soul, and worship of dumb ugliness, to secure success in the money market of a profession, which in its purity is too exalted for wealth either to nourish or debase. But if Keats had once tasted the true spirit of medicine, he would, I believe, have become one of her greatest sons,—an addition, perchance, to the some eight or ten of the men of all time whom medicine claims as her own, her poets of nature, like Keats himself, and in their way poetizing in undying thoughts if not in measured lines.

Here and there in Keats' letters, and more than once in his poems, the physician, touched, no doubt, by some metaphysical mysteries, stands forth in strong outline. His aspirations were the truest the truest physician could have. To his friend Taylor he writes, "I find that I can have no enjoyment in this world but continual drinking of knowledge. I find there is no worthy pursuit but the idea of doing some good to the world." To John Hamilton Reynolds he confides, "Were I to study medicine again, I feel it would not make the

least difference in my poetry; when the mind is in its infancy a bias is in reality a bias, but when we have acquired more strength a bias becomes no bias. Every department of knowledge we see excellent and calculated towards a great whole. I am so convinced of this that I am glad at not having given away my medical books, which I shall again look over, to keep alive the little I know thitherwards." To Benjamin Bailey he observes, "I am never alone without rejoicing that there is such a thing as death—without placing my ultimate in the glory of dying for a good purpose." Glorious Keats! what a real physician thou mightst have been had the Fates willed it.

There is another letter, addressed to John Taylor, in which a facility for subtle interpretation of mental origins of physical diseases is shadowed forth with singular keenness by our poet. "Our health, temperament, and disposition are taken," he observes, "more from the air we breathe than is generally imagined. See the difference between a peasant and a butcher. Agriculture is the tamer of men; the steam from the earth is like drinking their mother's milk—it enervates their nature. This appears a great cause of the imbecility of the Chinese. What is the cause of so many men maintaining a good state (of health) in cities, but occupation? An idle man, a man who is not sensitively alive to self-interest in a city, cannot continue long in good health. This is easily explained. If you were to walk leisurely through an unwholesome path in the fens with a little horror of them you would be sure to have your ague. But let Macbeth cross the same path, with the dagger in the air leading him on, and he would never have an ague, nor anything like it." Keats not a physician! why, the father of physic

himself might have written what the poet has here indited.

Moreover, though it be no credit, he had caught the style of the physic of his time.

“Physician Nature! let my spirit blood,”

is one of his lines, written, as Mr. Forman, in answer to a criticism on the wording, has shown, in the precise medical tone of a time when “let the patient blood” was as much a technical command in medicine as “right about face” is still a command in military drill.

Again, this poet of ours, for he belonged to us after all, this poetic prodigal, who might have returned to the medical fold, possessed another quality, which, added to his honesty of purpose and excellent learning, must have made him great there, I mean his astounding mental industry. That a youth of twenty-six years, barely, who had lost much time from lingering disease, should have left behind him such volumes of literature, to say nothing of the surpassing quality of it, is sufficient of itself to show what, during a ripe maturity spent in the contemplation of the phenomena of health and disease, he must have accomplished for the good of the human family and for the honour of his vocation. The poetic fervour, the power for subtle appreciation and interpretation of the bonds existing between man and the external eternal spirit by which man is animated,—in one word, genius, was in Keats concentrate. In him we held for a moment, and lost, not one “whose name was writ in water,” but the Hyperion of our era.



## OPUSCULA PRACTICA :

### A FEW USEFUL NOTES FOR BUSY PRACTITIONERS.

“ There are mites in science as well as in charity.”

BENJAMIN RUSH.

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### TEMPORARY PRESERVATION OF POST-MORTEM SPECIMENS.

**I**T is often important after making a post-mortem examination to preserve parts from decomposition without destroying their texture or natural appearance. I have found a plan of doing this which is as simple as it is effective. I take a glass bottle with a wide mouth and a large stopper. A bottle three inches in diameter and six inches long, with a stopper an inch and a half in diameter, answers well. I rub glycerine round the stopper, in order to make it fit evenly and correctly in the bottle. I remove the stopper and invert the bottle over a common coal gas-jet for a minute or two, until I find the coal-gas flowing over the side of the neck of the bottle, as detected by the odour of the gas. I then turn off the gas, and put the stopper into the bottle. I next place one fluid drachm of ammoniated chloroform into a measure, and removing the stopper quickly from the bottle, pour this solution into the bottle, quickly re-stop, and tie down the stopper.



The preserving bottle is now ready, and I usually take two or three bottles so prepared to the post-mortem chamber, in a small waterproof case.

After the post-mortem, the organ or part which has to be preserved, say the kidney or heart, is placed in a fold of soft muslin and gently pressed over the basin or sink, so as to remove as much fluid as is possible without injury to structure. The muslin is next puckered up to make a neck by which the specimen is enclosed as in a pouch or bag, and then it and the muslin are dropped into the preserving bottle, an assistant taking out the stopper for a moment, and replacing it so soon as the specimen is introduced.

If the stopper of the preserving bottle be now tied firmly down, there is no reason to be in any anxiety about the specimen for days, or even for weeks. If the bottle be put in a cold place, decomposition is quite prevented, and colour and natural structure are maintained.

In this method of preservation there are two factors. The ammoniated chloroform prevents the breaking up of the water of the tissues, that is to say it preserves; and the carbonic monoxide of the coal-gas prevents discoloration. After examining a specimen so treated, at leisure, the preservation can be repeated by the simple process of recharging the bottle.

In medico-legal cases, when the structure of the tissues has merely to be reported on, this method answers remarkably well. In cases of poisoning I should not recommend the use of coal-gas, because the impurities in the gas might lead to the suggestion that they were a possible source of poison, and the course of justice might thus be misdirected.

## AMMONIATED CHLOROFORM.

MMONIATED chloroform, referred to above as well as in the last ASCLEPIAD,—in connection with the bead inhaler,—is a mixture of ammonia and chloroform, which I am accustomed to administer, by inhalation, in some classes of disease. In the *Lancet* of June 9th, 1883, I described the preparation and its intention; but as some readers have made fresh inquiries concerning it, the following brief account is required.

Ammoniated chloroform is made by mixing together equal parts of the strong solution of ammonia in alcohol,—the old strong alcoholic solution of ammonia answers very well,—and chloroform.

When the ammoniated spirit and chloroform have been mixed for a short time, a little water floats on the fluid or adheres to the sides of the bottle in which the mixture is made, and can be removed by absorption of it by bibulous paper. The compound is then left clear and ready for use. It is a pleasant-smelling fluid, with much less pungency than ammonia, and it can be inspired without difficulty, from the inhaler, after a few minutes' practice. The patient can soon acquire the plan of administering it to himself or herself in the manner directed. I usually direct that two fluid drachms should be put, at one time, into the inhaler, and gradually breathed during half an hour, and repeated as may be required. The inhalation may be carried on until the narcotic action of the chloroform is clearly but not deeply manifested, and this condition may be maintained without danger, according to my experience, for several hours at a time.

The therapeutical intentions of the inhalation are,

to reduce febrile temperature, to relieve pain, and to maintain the alkalinity and fluidity of the blood. It is a method for reducing pyrexia by the process of inhalation—a method which will, in my opinion, be the only one employed for that purpose as the value of it becomes known, and which will, in time, become so perfect, that by it the reduction of pyrexia will be as definite and certain a procedure as the production of anæsthesia. I do not assume that ammoniated chloroform is the agent that will fully accomplish so signal a service; but it is in the right direction. Under its administration I have seen the febrile temperature reduced  $2^{\circ}$  F. in four hours, and  $4^{\circ}$  in twelve hours, and have sustained the reduction, by continuance of inhalation, until it has become permanent. Cases of puerperal peritonitis, acute rheumatic fever, scarlet fever, and septic pyrexia, are those in which the inhalation of ammoniated chloroform is most promising of good results.

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*ADMINISTRATION OF ALCOHOL PREVIOUS TO CHLOROFORM OR METHYLENE BICHLORIDE.*



I HAVE always practised the administration of alcohol previous to that of chloroform or methylene bichloride, and I have no reason to doubt the propriety or advantage of the proceeding. Snow, although his researches did not extend to the action of alcohol, was inclined to endorse the view that by administering a dose of alcohol, by the mouth, a short time before the inhalation of chloroform, the second degree or stage of chloroform narcotism, the degree of convulsive excitement, was bridged

largely over, and that, possibly, vomiting was somewhat controlled. This is really the case; and when I was actively engaged in chloroform and methylene administration, I got at last to the art of reducing the second and dangerous stage to a minimum, by alcohol.

The reason is clear. The excitement produced by all the anæsthetics of the chlorine series is not due to the radical, which is the real narcotic, but to the chlorine associated with the radical. The chlorine gives weight and stability to the fluid, and makes the fluid very manageable; but, physiologically, it is actually mischievous. It excites and it causes contraction of muscular fibre generally, involuntary and voluntary. To this contraction alcohol is antagonistic, and so when alcohol has diffused through the body to the degree of causing relaxation of the minute vascular canals, the condition is favourable for chloroform or its allies.

The method of administering the alcohol by the mouth, instead of mixing it with the anæsthetic and giving the two together by inhalation, is soundest, because its diffusion is then more equal and precedes the diffusion of the chloride vapour, a matter of considerable importance, inasmuch as time is allowed for the alcohol to produce its specific relaxing effect upon the minute circulation before the opposite action of the chloride comes into play.

In using the word alcohol in the above observations, I am referring, I wish it to be understood, to the thing itself, not to any other liquid, like brandy, rum, gin, or wine, that may contain it in any unknown quantity, and be admixed with any unknown substance. There is neither sense nor science in making use of these untrustworthy mixtures.

The administration, to be reliable, should consist, for

an adult, of a draught containing from four to six fluid drachms of pure ethylic alcohol of sp. gr. 830. The alcohol should be taken in eight ounces of water, at a temperature of 95° to 100° Fahr. It should be taken twenty minutes before the chloroform or methylene is administered, that is to say, at the time when the rapid pulse, slightly increased warmth of surface, and flush on the skin, indicate that the first stage of alcoholic toxic action is being manifested.

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*USE OF PEROXIDE OF HYDROGEN FOR THE DIAGNOSIS  
OF PUS FROM MUCUS.*

INCE the time when I introduced peroxide of hydrogen into medicinal use,—in 1858,—it has been applied in various ways for various purposes; but there is one application which has never come into practice, although at a very early period I made a point of impressing it as a serviceable practice. I refer to the employment of the peroxide solution as a means of diagnosing pus from mucus, and of the presence of pus in mucus.

Some will remember that in the original research with the peroxide I found that certain structures of the body, when placed in the solution of peroxide, dislodged the oxygen, while other structures did not. In this I followed up what Thénard, who discovered the peroxide, had done in respect to various other substances, organic and inorganic. The results were of peculiar interest. Mucus brought into contact with the solution does not affect the solution in the least, if the mucus be free from carbon-dioxide; but the



smallest amount of purulent matter at once liberates the oxygen freely. A vaccine point dipped in the solution instantly sets free bubbles of oxygen. Albumen does not affect the solution. Fibrin immediately sets free the oxygen. Blood corpuscles under some circumstances do, under other circumstances do not, set free the oxygen, the probabilities being that the red corpuscles do not, and the white do. Ordinary fibrous tissue does not set free the oxygen; cellular tissue liberates it with violence.

To apply these observations to the detection of pus in excretions, as from the bronchial surface or the bladder, I place four drachms of the solution of the peroxide, of ten volumes strength, in the test tube, and add the suspected secretion. If there be escape of gas, it is fair to assume that pus is present. If gas does not escape, the fact that pus is not present may be accepted as certain.



*AN INDIA-RUBBER DOUBLE STETHOSCOPE.*

HE late Dr. Robert Willis gave me, in 1850, a stethoscope which had belonged to a pupil of the great Laennec himself, and that stethoscope has been of daily service to me for over thirty years. It had been in use almost as long a time before it came into my possession, so that it may now have an age of over sixty years. It is as good as ever, and I never wish for a better. It is a tube of wood, finished off with an ivory ear-piece, but it is troublesome for the pocket.

To make a convenient pocket-stethoscope, I got the



Messrs. Weiss once to construct for me a tube with an ear-piece which would turn down when the instrument was out of use; and by a simple joint this was neatly effected. I afterwards added a reflector to the ear-piece, and adapted a small ear-speculum to the mouth of the tube. The whole formed a very complete instrument, useful as a reflector as well as a stethoscope; but it was apt to break in the pocket, and was not quite so convenient as I expected it to be.

For a few years past I have always carried a simple double stethoscope, made with two lengths of vulcanized india-rubber tubing, fitted for the ear with ivory tubes, which could be held in the ear, and completed at the other end with a simple wooden mouth-piece. By practice, I have learned to hold this instrument without any spring or other support, and now I have made it very perfect by having it produced entirely in india-rubber. The ear-pieces, the tubes, the mouth-piece, are all one common piece of vulcanized structure, pretty firm, but so elastic that it can be carried without any inconvenience. It conducts sounds well; the india-rubber ear-pieces adapt themselves comfortably to the ears; and, in whatever position the patient sits or lies, the instrument is convenient.

By holding the mouth of the instrument close to the chest by the first and second fingers of the left hand, and by percussing on the fingers while the ear-pieces are in the ear, the process of percussion is made exceedingly delicate.

Another advantage of the double india-rubber stethoscope—minor, it is true, but not without its value—is that it can be carried in the pocket without the slightest annoyance or the fear of breaking it on sitting down.

*CRUSHED ICE AND LARD IN THE TREATMENT OF  
BURNS AND SCALDS.*

**H** WAS called in consultation to see a youth who had been severely burned on the surface of the abdomen, by the explosion of some fireworks which he was carrying in his trousers' pocket on Guy Fawkes' day. He was suffering excruciating pain, which was relieved by nothing but the application of cold water. As the water caused much damp, I suggested, instead of it, finely crushed ice, in a thin cloth bag coated with olive oil. The plan answered excellently, no further pain was felt, and the open surface, extending over half the abdomen, healed most kindly.

At first I thought the practice was new, but I have since found, in Sir James Earle's admirable essays, that he anticipated me. Earle, indeed, had a better plan than mine. He mixed the crushed ice with fresh lard, and obtained results of the most satisfactory character, which I can confirm.

To put the method into practice, ice is well crushed or scraped, as dry as possible, into the finest division. Then fresh lard is admixed until a broken paste is formed. The mass is put into a thin cambric bag, laid upon the burn or scald, and replaced as required. So long as the ice and lard are melting there is no pain from the burn, and so long the application is serving its purpose. The return of pain is the call for the repetition of the remedy.

I do not think any one who has seen this beneficent and efficient mode of treating burns and scalds carried out a few times will forget its value. It is as scien-

tific as it is simple. It saves at once the fever incident to pain, and it leaves very little contraction of surface; two points which, of themselves, are greatly in its favour.

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*INTERNAL ADMINISTRATION OF AMYL NITRITE.*

**I**N introducing amyl nitrite into medicine in 1863, I recommended its administration by inhalation, a plan which has been generally followed by the profession. For some years past, however, I have found it advantageous, in some cases, to administer by the mouth. The most convenient formula is as follows :—

Amyl nitrite, pure, m. xxxvi.

Ethylic alcohol (sp. gr. 0·830), ℥vi.

Pure glycerine, to ℥iss.

To make a mixture of twelve doses. One fluid drachm for a dose in a wine glassful of warm water.

The dose should be swallowed slowly, and may be repeated every two or three hours, if necessary. The relaxation produced is often longer continued than when the same dose is inhaled. In asthma, specially, I think this an improved method. It relaxes the spasm without causing interference with the entrance of pure air into the lungs.

*OBSERVATIONS ON MR. EDWARD PAYSON  
WESTON AFTER HIS WALK OF FIVE THOU-  
SAND MILES IN ONE HUNDRED DAYS.*

*A STUDY OF VITAL MEASUREMENTS.*

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INCE March the 18th, 1884, I have had the opportunity of making two careful physical examinations of Mr. Edward Payson Weston, who between the hours of nine and ten o'clock on Saturday evening, March 15th, 1884, completed, at the Victoria Coffee Hall, near Waterloo Railway Station, London, the very remarkable feat of walking five thousand miles in one hundred days. The walk thus accomplished was carried on partly out of doors on common roads, and partly on covered tracks ; part, that is to say, up hill and down dale in all weathers belonging to the months of November, December, January, February, and of March, and part on a level plane, under shelter. The out-door walking was, of course, most exposed and most laborious, but the indoor was not without its drawbacks, the chief of which was the shortness of the laps. In one track the laps were thirty-six to the mile, and made the pedestrian so giddy

at the first start that he was brought nearly to a standstill. At night, also, after a day of these short laps, he continued to suffer. He experienced a sensation as if he were turning a somersault, forwards, just as he was going off to sleep, a phenomenon which would recur several times before he was fairly rocked into sleep. The impression on the nervous system was much like the impression of the motion of a vessel at sea on some persons, after landing from a voyage.

During the effort, Mr. Weston rested regularly each Sunday, and on Christmas Day. He did this from a desire not to cause objection to be felt respecting his task, rather than from any necessity for the rest itself. On the contrary, he would, as a matter of exercise, have preferred to have continued at his task, and have finished eighteen days sooner. The Sunday rest affected him little on the following Monday, but on the succeeding Tuesday morning it caused each time a drowsiness which impeded him more or less throughout the whole of the day, and which he attributes to the circumstance that he did not get his usual sound sleep on Sunday night.

In the entire walk, Mr. Weston made nearly 11,000,000 steps. He averaged three and a half miles to four miles an hour. He generally rested from two to three hours during the day, but not always. On the last day he walked from Brighton to London, 53 miles, without resting at all; he did not even sit down, but took food and drink as he went along. He finished each day's walk by delivering a lecture, and at the close of his feat he seemed to be as fresh and vigorous as ever.

These in very brief terms are the broad facts of the work Mr. Weston accomplished in the one hundred days of his labour. Let me now turn to the organi-

zation that did this labour in a physiological and, it may almost be said, in a clinical point of view.

#### GENERAL PHYSICAL CHARACTERISTICS.

Mr. Weston is by birth an American. He is forty-five years of age. His height is five feet seven inches and a quarter. His naked weight is 9 stone, or 126 pounds. His eyes are greyish blue. The hair of his head is light brown, his moustache sandy. His complexion is fair. His temperament is nervous-sanguine. He does not recall that there is any hereditary disease on either side of his family. His father died of typhoid fever at the age of sixty-two. His mother was often ill, but was of great endurance and courage; she died at sixty-nine, somewhat suddenly, from senile decay rather than from any definable local disease.

Mr. Weston's habits through life have been those of a very active and temperate man. He has several times entered into contests of a pedestrian nature, and in 1870 attempted the feat of walking 400 miles in five days, under the observation of the distinguished physiological professor, Austin Flint, junr., of Bellevue Hospital College, New York. Then and on other occasions when he has walked against distance, or time, or both, he has totally abstained, a month before and during the effort, from all alcoholic drinks. He has not, however, until recently, been at other times a total abstainer. In 1870, Professor Flint reported of him, that "though not what is called a total abstainer, he is not an habitual drinker. He occasionally takes a glass of ale or wine, but this is rare." He continued the same course until three years ago, since when he has followed total abstinence altogether. He is a smoker of what some would consider a moderate type. He



smokes more or less in turns. I should say sometimes overmuch, but never much while engaged in one of his extraordinary efforts.

Mr. Weston has enjoyed very good health all his life. He had two weeks of "feverishness," perhaps from cold, in 1861. He had slight rheumatism in one foot in 1863. He had vertigo after a severe test of walking, while suffering from cold and headache, in 1870; he had excessive vertigo, or giddiness, with staggering and inability to see the track properly, on the fourth day of his walk against time in 1870; but he quickly recovered, and walked forty and a half miles on the following day. He attributes the last-named giddiness to deficiency of sleep, which may be accepted as a perfectly just explanation. In the fall of 1872, he had a diphtheritic ulceration of the throat, in the United States; and in the three following years, in the same period of the year, he had quincy. On coming to England these attacks of the throat have never occurred; but in 1880 or 1881, being then in the United States, the quincy returned as before, at the same season as before. He can remember none other derangements of health than these. He has never suffered acute pain, and his appetite is good.

In regard to sleep, he is, in every sense, a good sleeper. He drops off quickly when he retires to rest, and ordinarily sleeps soundly from eight to nine hours. During his great feats of endurance, he likes to have no more than seven hours of sleep, and he invariably, under such circumstances, wakes at the end of six hours. His experience, and this is very important, is that seven hours is the proper time for the best work of an extreme physical nature. In some of his walks he has slept for much shorter periods, but has then never done so well.

## MENTAL CHARACTERISTICS.

The mental characteristics of Mr. Weston are well marked. He is full of energy, rapid in thought, quick in perception, and acutely sensitive. With these qualities he possesses the most astonishing self-reliance in what he undertakes to do, and endurance in carrying out what he determines upon. He is ready to submit himself to physical tests which seem impossible, and except from some physical failure, practically mechanical in its character, he carries on to the end. His mind never knocks under, and when a public voice called out, while I was speaking of him at a large meeting, "It's all pluck," the voice speaking for the public was largely correct. When he is in the midst of his work he is, however, easily made irritable by anything that goes against the grain. Dr. Flint observed this fact in 1872, and records it by saying, "While walking, Mr. Weston was excessively sensitive, and was disturbed and annoyed by the slightest things." I noticed the same sensitiveness during the walk which ended on March the 15th of the present year. At the same time, he is most kind to those who are in communication with him, and submits to rules and examinations with a willingness I have never seen surpassed. He has a very tenacious memory.

## SPECIAL PHYSIQUE.—MUSCULAR SYSTEM.

Mr. Weston cannot be said to be strong physically, in the full meaning of that word. I agree with Dr. Flint, that "his general build is slight, and the parts above the waist very light." The girth of his arm, at the full diameter of the biceps, is  $10\frac{1}{2}$  inches on the left,  $10\frac{3}{4}$  on the right side. The strength of his upper limbs is comparatively slight. The readings from the

dynamometer, taken twice daily for five days, varied for the right hand from  $75^{\circ}$  to  $110^{\circ}$ ,—the average power from all the readings being  $100^{\circ}$ ,—for the left hand  $70^{\circ}$  to  $105^{\circ}$ —the average being  $95^{\circ}$ . These are comparatively low readings. A man in his prime, who is strong in the muscles of the forearm and hand, can produce with the left hand  $130^{\circ}$ ; with the right,  $140^{\circ}$ .

The trunk of the body is well shaped, but not large. From acromion to acromion, anteriorly, is 16 inches. From the angle of one scapula to the other, with the arms folded as tightly as possible across the chest, is 9 inches. From nipple to nipple is  $8\frac{3}{8}$  inches. The length of the sternum is 8 inches. The upper girth of the chest, tape passing immediately under the armpits, is  $32\frac{1}{2}$  inches; the middle girth, in line of nipples,  $33\frac{1}{2}$  inches; the lower girth, at level of base of sternum, is  $30\frac{1}{2}$  inches.

The girth of the abdomen at the level of the umbilicus is  $29\frac{3}{4}$  inches.

The lower limbs are more fully developed than the upper. The pelvis is large, and the muscles of the thighs are still, as Dr. Flint observed of them fourteen years ago, well developed. The muscles of the leg are not so large. The girth in the greatest diameter of the calf is  $13\frac{1}{2}$  inches only. The foot is very well formed; the arch a little depressed, but fair; the heel long, the toes well shaped, and scarcely overlapping at all. The length of the heel was observed upon by Flint, with much acuteness, as "giving remarkable leverage for the tendo achillis."

Mr. Weston drew my attention, as he had previously drawn the attention of Professor Flint and other observers, to the fact that his muscles are always soft, even when he is in the best walking condition. He observed to

Dr. Kerr, Mr. Blyth, and myself, that the soles of his feet also remained quite soft and pliable after he had been on foot all day in walking from Brighton to London. He has never known his muscles "hard" or "fine," as in trained athletes. "It has seemed to me," observes Flint, "that this peculiarity of the muscles is advantageous. When the muscles are very hard from thorough training, prolonged exertion is apt to produce cramps, due, perhaps, to an exaggeration of the normal irritability." I entirely agree in this view, as well as in the further view expressed by the same author, that this suppleness of muscle is probably the reason why Mr. Weston scarcely ever feels "leg tired." He assured me after the walk from Brighton without sitting down, a walk of 53 miles, that he had no leg weariness whatever. We meet with this same peculiarity sometimes in the horse as well as in the man ; and the old saying, "over strained is over trained," has, no doubt, like many such terse notes, had its origin in the simple recognition of an important physiological truth.

#### TEMPERATURE, RESPIRATION, AND PULSE.

Mr. Weston's temperature varies very little. His constant may be taken at  $98\cdot2^{\circ}$  when he is not performing any out of the way muscular work. In the last five days of the long walk it was  $97\cdot6^{\circ}$  F. in the early morning, rising during the day, and attaining in the evening  $98\cdot2^{\circ}$  F. It never exceeded  $98\cdot2^{\circ}$  F.

The respirations, normally, are 20 per minute. During rapid walking they would rise up to 28, but in a few minutes after resting, they would fall to 20. The average during the last few days of the late walk may be taken at 22, the ordinary at 20.

The pulse in the usual state, and in the sitting position, is 72 beats per minute. When he was walking gently, not over 4 miles an hour, it rose to 80. If he were excited or worried, it would rise to 90, and under extreme rapidity of walking it would suddenly run up to an extreme height. When he walked a mile in 10 minutes, the pulse rose to 134. When, with much excitement around him, he walked a mile in 9 minutes 17 seconds, the fastest mile he ever walked, it reached 176 beats per minute. At the immediate close of the 53 miles walk from Brighton, I counted it as 126, but there was then considerable mental worry, brought on by a narrow escape from an accident to some one in the crowd which he had noticed, and which affected him very severely for a short time. The rapid rise of pulse was quickly over on all occasions so soon as quiet was secured.

### *Respiratory and Circulatory Records.*

Mr. Weston's vital capacity of chest, as tested by the spirometer, varies from 200 to 222 cubic inches. The maximum is 222. The resonance on percussion all over the chest is clear, and the resonance increases a little over the cardiac region, as if the structure of the left lung enveloped the base of the heart rather more than is usual; the respiratory murmur is everywhere perfectly clear, and the chest fills equally and evenly. The vesicular murmur is slightly louder on the left than on the right side, but the difference, which is only detectable by a long process of comparison, means no more than the disparity which is often met with in healthy people, and which indicated a trifling difference of development between the two breathing organs.

The cardiac region of dulness is rather small, but in



correct position. The heart is of moderate, I should say, natural, size for the body. The action of the heart is very regular, and the systolic and diastolic sounds clear. The impulse is natural in respect both to position and quality. The pulse readings, sphygmophonic and sphygmographic, are good. The sphygmophone indicates, perhaps, a slight shortening of the third sound: The sphygmographic tracings, of which several have been taken, give evidence of a good systolic stroke with a recoil slightly prolonged, and an aortic closure wanting somewhat in firmness. This is exactly what would be expected in a man at forty-five years of age who has put so severe a strain so often on the semilunar valves and the base of the aorta. The position of the body which gives the best tracing is the sitting position. The variations in the three positions, of standing, sitting, recumbency, are very pronounced. In the same period of time ten strokes of the heart in the standing position are represented by eight in the sitting, by six in the recumbent. The up stroke is shortest in the standing position, longest in the sitting. The descending stroke, down to the notch, is longest in the sitting posture. The descending stroke from the notch to the commencement of the ascending or systolic stroke, corresponding to the period of cardiac rest, is longest when the body is recumbent. I consider this to be in entirely natural order.

#### THE ABDOMINAL AND RENAL SYSTEMS.

The external examination of the abdomen gives evidence of a healthy condition of the abdominal viscera. The bowels act, as a rule, once daily, and the excreta, natural in character, weigh from five to six ounces when the body is not undergoing any unusual effort.



The renal system is normal in action. The amount of urine passed, under ordinary circumstances, is natural in quantity and quality. The quantity averages 50 fluid ounces daily : reaction, acid : colour, natural : deposits, rare and never copious : sp. gr., 1.022, at 60 F. : urea, 620 grains daily. The urine free from sugar and albumen may be considered quite natural.

#### THE CUTANEOUS SYSTEM.

The skin is free from any special mark or growth, and is pale and soft in structure. It would be called, in ordinary language, a delicate skin. The perspiratory action is moderate under exercise, but not excessive under violent exercise. The sensibility, tested by Dr. Sieveking's æsthesiometer, is natural in all regions.

#### NERVOUS AND SENSORY SYSTEMS—EXPRESSION.

There is no indication of any nervous failure or irregularity. The nervous control over the minute circulation is, no doubt, easily disturbed by emotional shocks and irritations ; but the balance is soon restored, and the organic condition may be considered sound. The vision is good, and the test-types of print can be read at their proper distances. The hearing is good on the left, not quite so good on the right, side. Tested by the audiometer, Mr. Weston could follow the balance down to 10 units from zero by the left, 26 by the right ear. Taste and smell are in a good state, and touch is very delicate. The facial expression is that of a man within the age of forty-five years.

#### SUMMARY.

It will be gathered from this record, that, in so far as current means of diagnosis assist us, Mr. Weston stands before us a specimen of a man of as good a standard of

health as we could expect to see. I do not remember ever before to have devoted so much time to the exhaustive diagnosis of any one healthy person; but tedious as the labour has been, I do not regret it, since it has given me, and I hope may give others, a few hints for a mode of arriving at a standard of vital measurements. A set of standards of such measurements being supplied, we ought, by the application of the exclusive method, to make diagnosis an art of absolute certainty in most cases of disease.

In the matter of diagnosis, Mr. Weston has afforded ground for useful observation. But it is to physiology that he has been most useful as a subject for experiment. In this direction he gave Dr. Flint, and Dr. Flint's learned colleagues, the opportunity of bringing out, in 1870, the best report in any language on the vexed question whether, during muscular exercise, the muscular substance itself is consumed, and whether there is indication of such consumption being indicated by the increased elimination of nitrogen by the urine; or whether, during muscular exercise, it is only the fats and non-nitrogenous foods that are consumed, and that the excess of nitrogen is due to the amount of nitrogen taken in as food, not to actual consumption of the muscular fibre itself.

How far this most important question is further answered by the results of Mr. Weston's latest pedestrian feats I leave for a future study; but there is one physiological truth which I must note, as I conclude, namely, that the healthy condition which he represents after one of the severest physical trials on record affords the most decisive of proofs of the success of meeting physical work on good diet,—sans wine, sans beer, sans grog, sans everything of the toxic sort.

## COTEMPORARY PRACTICE AND LITERATURE.

*“ Every physician will and ought to make observations from his own experience ; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together.”—FRIEND.*

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### PESTIS BACILLORUM.

“  IOGENES rogatus, quemadmodum sepeliri vellet, projici se jussit inhumatum. Tum amici: Volucribusne et feris ? Minimè vero, inquit: sed bacillum propter me, quo abigam, ponitote. Qui poteris ? illi dixerunt ; non enim senties. Quid igitur mihi ferarum laniatus oberit, nihil sentienti.”

Would that Diogenes, who wanted a real bacillus to be placed by him when he lay dead on the open plain, so that with it he might drive off the birds of prey and wild beasts, were here now to drive away the “ bacillus omnivorus ” which is eating into our moral, if not our physical, vitals with so much vengeance. What have we done to be visited in the heavens above, in the earth beneath, and in the waters under the earth, by the “ Pestis Bacillorum ” that is now regnant ? Diogenes present in these days would suffer from bacilli everywhere ; in his food of all kinds, in his teeth, in his stomach, and in his tub by millions. From them Diogenes would be subjected to cholera, consumption of the lungs, epilepsy, typhoid and every other sort of

fever, syphilis,—a new idea to him,—and child-bearing, in course of time, as a matter of necessity, and only to be waited for. Seriously, this bacillus business is becoming a painful interpolation. A young lady whose grandmother, two aunts, and mother died of consumption, who had a pigeon-shaped chest, and what we poor ignorants, who cannot swallow bacilli willingly, would call a strumous constitution, took a severe cold, and from that day became consumptive, spat blood on the fifth day after the chill, and, passing rapidly through all the stages, died of phthisis pulmonalis. It is herewith to be thrust on us that she died from the bacillus of tubercle. All the preceding clinical history must be treated as nothing in presence of bacillus. In a district in India, Berar, two people die in one year of cholera. In another year, in the same place, by a sudden sweep of the pestilence, 22,465 die. We are asked to accredit that in such an outbreak the disease is due to the development or transmission of a bacillus, which has suddenly taken occasion to increase and multiply without any provocation. We are asked to accept, tentatively, that rabies is from a microbe. We wait, and then we are told that there is a virus, as if that were a great discovery, but no microbe. A skilful inventor suggests to us that there are some bacilli which are inimical to other bacilli, tribes savage and warlike, opposed to other tribes; and we are invited, as our forefathers might have been invited to a cock-pit, to see this ingenious man make a bacillus-pit of the human body, and set bacillus at war with bacillus, until they exterminate each other.

Can this greedily absorbed hypothesis come to any good? Upon the evidence of how many or how few men does it rest? On what reasoning does it rest?

Who has separated, in relation to it, coincidence from causation? To what does it promise to lead? The last question is the most important. If the hypothesis leads much further, it must bring back the exploded doctrine of diseases as entities; of diseases not as simple perversions of physiological states but as existences, having each one its own germ and growth and life and death. If epilepsy be caused by a germ passing into a bacillus, why may we not return to the possession doctrine, and say of every epileptic that he is possessed of an evil spirit, a devil of bacillus origin? And mark! to what the speculation must also bring its reasoning follower, if he be consistent, in regard to origins of men and animals as such. In one secretion there are spermatozoids. Do they come from germs floating in the air, which the male subject appropriates and the female rejects? Can they be cultivated in a gelatinous or other proper menstrum? Would they, so cultivated, produce a disease, say syphilis, as an entity, or if not, what else?

As matters now stand, we have either before us a revolution in discovery, momentous in character, or one of the most dangerous of speculations that ever was revived out of the mad past,—Dwight's animalcular hypothesis with less excuse for it. Strangest that many who treat upon the subject, as if they had in their possession the explanation of explanations, do not in their hearts believe in it, but consider it as a fashion which must be accepted in order that they may not be peculiar. To me, I confess it is a solemn thought, that as by a sudden incantation the grand experiences of medicine and assured beliefs of it should be thrown so jauntily aside by so many, in order to let in a speculation which may easily be resting for its basis on nothing more than a



series of phenomena purely coincident in character, and having no relation to cause or causation. That as an addition to natural history, as an addition to the parasitic history of disease, the bacillus controversy may ultimately be of service, all candid minds may admit; but these wild cries of discoveries of causes, of which we hear now every week, which fill the minds of the people with hopes of universal relief from the great pestilences, and which prove falsest to such hopes by diverting the mind from direct practical and acknowledged plans of prevention, are calculated to breed more distrust of medical science and art in the future than aught that has ever occurred in any period of the past.

THE ADMINISTRATION OF CHLOROFORM BY PER CENTAGE  
MEASUREMENT OF VAPOUR.

The general as well as the medical press has been rich in comment, during the past quarter, on the researches of our able and most distinguished French brother, Paul Bert, into the subject of the administration of chloroform in measured proportions of four per cent. vapour in ninety-six per cent. of common air. The research, as an old one here, possesses no novelty. Before chloroform was discovered, the late Dr. John Snow had commenced to experiment with ether in order to ascertain whether by accurately determining the per cent. of ether vapour, in air, that was sufficient to narcotize, a fixed rule of safety could not be laid down; and I have still in my possession an ether inhaler, with thermometer and reservoir invented by Snow, in 1846, for this determination. When chloroform came into use, Snow immediately followed up the research in its application to chloroform, and invented a very simple and practical inhaler, which would never



give out more than five per cent. of chloroform vapour. I have the case-books of Snow, which came to me when I was writing his life and editing his last work in 1859, in which are recorded 4,285 administrations of chloroform under his method without a death. I followed it myself for several years with the same success.

After Snow, Mr. Clover took up the same system, with a little modification. Snow had originally invented what he called a balloon, which was, in fact, an elastic bag of balloon shape, into which he blew with a small pair of bellows so much air and so much chloroform, taking care that the air diffused through the balloon should not contain more than five per cent. of chloroform; but he afterwards gave up the apparatus on the ground that it was cumbersome, troublesome, and apt to leak. Clover revived the balloon as a bag, and, by great pains, brought it to much perfection. The material of which it was composed, silk lined with an impervious coating, was very light and very enduring. The administrator carried the bag on his back like a knapsack. I used Clover's bag many times, and have often seen it in use. It was, I believe, more accurate than Snow's inhaler.

It will be wondered at by some why this method of administering chloroform went out of use, and has had, so to speak, to be re-proclaimed. There is no cause for any wonderment on the subject. The method ceased because the merit which it claimed for itself was forfeited in practical work. It is the same in these things as in social affairs. If a man with a good name trips up somebody,—though it be by the very fault of the person tripped up,—he will be scouted more than the scoundrel who has systematically devoted his life to the tripping up business. The mischief was not

expected from the trusted man, and so when it came it was an event; it was always expected from the scoundrel, and so was of no consequence. The simile holds. Snow's plan, for it is entirely his, was so sound, rational and exact that it brought the dangers of chloroform to their minimum; but it did not absolutely prevent them, and when death did follow administration, as it did both from the inhaler and the bag, confidence in both was destroyed. Why death should occur under a method which seems so safe, is also easy to understand. Snow discovered that when healthy lower animals were narcotized by four per cent. of chloroform vapour, they were as safe as when they were breathing common air, if the administrator were but reasonably cautious. He made this a matter of demonstration many times, and he was so certain upon it that he felt he could submit it to the strictest proof of experiment. From this he naturally enough drew the inference that the safety he had secured ought to extend from the lower animals to man. That inference was, as experience has proved it to be, wrong. Men and women are not like lower animals in respect to anæsthesia. Children are nearer the position, but even they are different.

When we administer chloroform to members of the human family, without selection, we find that four per cent. of chloroform vapour in air is the minimum dose that will suffice to cause surgical insensibility. That dose will, usually, answer, although a lesser dose will not. So far all is straight and clear. But now comes the difficulty and the danger. In a population varying from two to three thousand, there will generally be found some person, *morituri*, ready to die from the specific action of the vapour upon his or her nervous vascular

organization. To such *morituri*, per centage makes little difference, because enough to anæsthetize is enough to kill. In fact, such persons are not fitted to approach so near to the gate of death from chloroform anæsthesia as to be saved from passing through it. To these persons, Snow's inhaler, Clover's bag, and Paul Bert's apparatus, however nicely adjusted they may be, will be fatal. The primary danger lies in the condition of the patient; the secondary in the chlorine of the anæsthetic. That is the gist of the whole question.

#### THE COLD BATH TREATMENT OF ENTERIC FEVER.

If my memory does not betray me, Dr. Currie, of Liverpool, in the year 1805, brought out his once famous practice of the cold bath treatment of fever. Currie did not know the specific differences between typhus fever and enteric, and it is probable that typhus cases were chiefly included under the term fever as he used it. He appears, however, to have produced good results, and the question he raised created, I believe, at the time an interest in professional circles which did not die out for a quarter of a century or more. Between 1840 and 1850, when Preissnitz was in his glory, the subject came up again, modified by the wet sheet packing system of applying cold to the body in fever, but soon died out. In the past quarter the cold bath treatment, in relation to "enteric fever," has once more been brought into prominent notice through Dr. Sidney Coupland's extremely able paper, and Dr. Bristowe's admirable comments upon it. In brief, Dr. Coupland's arguments in favour of the cold bath treatment are, that "if it do not exert any control over the intestinal lesions, it does appreciably diminish the dangers to life due to pyrexia itself, and places the

patient in a better condition for recovery. Nervous symptoms are allayed, sleep is favoured, delirium is banished or much reduced, the prostration is less, the so-called "typhoid" state is conspicuous by its absence, the pulse gains in force, the tendency to pulmonary congestion and collapse of the lung is obviated, the digestive and assimilative functions are improved at the close, the patient's mental and physical vigour is more speedily restored, and such *sequelæ* as mania and other conditions indicative of profound nutritional disorder of the nervous system rarely occur." Such a list of good services demands the most earnest attention; indeed, it is difficult to conceive any practice more deserving. True, it is only secondary treatment, a check to the bad effects incident to long increased temperature, not to the cause of increase; but that, though it be not basic, is useful advance. The only question I would raise is, why Dr. Coupland holds by such a clumsy and disagreeable method as the bath when, with Leiter's pliable metallic tubes, he can secure the same reduction of temperature without removing the patient from bed? I found, at the time when I introduced the cold water neck pad, in which cold water circulated through a pad round the throat, that I could reduce the temperature, from one to two degrees Fahr., by that alone. By the extension of the same plan, through Leiter's tubes, to the abdomen as well as the neck, any required reduction should be obtainable in the readiest manner.

#### INTRA-PERITONEAL INJECTION.

With a candour and kind acknowledgment it is not common to receive, Mr. Cameron Macdowell refers, in a letter to the *Lancet* of February 2nd, to my original

method, published in the years 1854-56, on the treatment of cholera by intra-peritoneal or vesical injection. I am quite sure that Mr. Macdowell's suggestion was made without any knowledge of these previous experiments and experiences, and it will perhaps be best for me to write a revised report of the original research in a future number of the ASCLEPIAD, for the purpose of giving its details, and affording readers the opportunity of testing its practical value. Briefly in this place I may state that three methods of introducing water into the body during the collapse of cholera were tried—(a) by the peritoneum; (b) by the cellular tissue; (c) by the bladder. Of the three the peritoneal injection answered best in every way. I discovered that by the peritoneal surface water was freely taken up into the blood, and that until an absorption of water equal to a fifth part of the weight of the animal was reached, no danger ensued. Any excess over a fifth, caused, in the lower animals, comatose symptoms, which usually proved fatal. In one case of cholera in the human subject, I injected the peritoneum with water at blood temperature without difficulty and without causing the patient the least pain or discomfort. The recovery from the collapse was rapid and complete; but I have never considered that this one case was of value beyond affording indication that the operation might be carried out without danger. I believe still that the principle is sound and worthy of adoption in practice.

#### PROCEEDINGS OF THE PHYSIOLOGICAL SOCIETY.

The resolution of the Physiological Society to publish a half sheet of Proceedings in regular series is a step in the right direction, and promises well. In the three first parts there are abstracts of fifteen papers read



before the Society. Dr. McWilliam has a demonstration concerning "The physiology of the heart of the eel." Mr. Victor Horsley contributes a record of "four cases of injury to the brain in man, illustrating very exactly the position of the cortical motor centres in the human cerebrum." Dr. S. Ringer supplies a communication on "The influence of saline media on fishes." Dr. Mortimer Granville communicates on "Nerve vibration and nerve excitation, or stimulating percussion." Dr. E. Klein follows with a notice on the "Bacteria of swine plague." Dr. Pye Smith gives a demonstration, in which the effects on the ear and pupil—in the rabbit—of excision of the superior cervical ganglion had persisted for over five years. Messrs. Langley and Sherrington describe sections of the medulla oblongata and spinal cord of a dog, in which degenerations from synthetic experimentation were presented. Miss Greenwood, through Mr. Langley, furnishes a paper on the "Gastric glands of the pig." Mr. Charles S. Roy offers a note "On a method of measuring the specific gravity of the blood for clinical use." Mr. Wooldridge makes some remarks "On the coagulation of the blood."

I supply these details in order to convey to my readers some idea of the useful work which the Physiological Society is supplying in a short and practical form, and I wish I could refer at length to several of the essays named. I must be content to make a note on one important observation by Dr. McWilliam, viz., that by the passage of an interrupted current through any part of the auricle of the heart of the eel, the action may be inhibited, while the remaining part of the auricle goes on beating. In warm bloods I have noticed a similar fact, and have observed, further, that by the interrupted current both the auricular and ventricular contractions



can be arrested. From that I have inferred, and have pointed out in my experimental lectures on death by chloroform, how dangerous an agent the interrupted current is in attempts at resuscitation after collapse under chloroform. Dr. McWilliam's new record leads me to repeat this caution still more emphatically.

“THE PEDIGREE OF DISEASE.”

“The Pedigree of Disease,” by Mr. Jonathan Hutchinson, F.R.S. (London, 1884, Messrs. J. & A. Churchill), is one of those books in which, in every page, it is manifest that the author of the book observes and thinks, and observes better than he thinks. Always drinking in knowledge, he writes from a store of original information which very few men possess. This is the value, intrinsic and original, of his writings, rendered in a style easy, clear, and pointed, all of which virtues make us, who read attentively from beginning to end, desire one better part still,—a wider or a more widely-expressed knowledge by the author of his own repertory or treasury of knowledge. It may be a desire to condense, it may be a deficiency of time for literary development, or it may be a habit of a half-philosophical exposition of a subject with a desire to make every attempt at exposition bear on the practical work in which the author is engaged, that interferes with the fulness and completion of what he describes. But whether it be any of these or anything else, there is something in this effort, as in others from the same hand, which renders the work incomplete and does the writer of it self-injustice.

When I say this it is with all admiration, prompted only by the lesson of one of our greatest of the greatest masters : “ Though friends and truth be equally beloved,



After detailing a number of experimental researches bearing on the question of the appearance of uric acid in the urine of one animal and of urea in another Dr. Latham proceeds to observe: "The appearance of uric acid in the secretion of one animal and of urea in another, is the result primarily of the non-transformation or metabolism of glycocine into urea;—whether that glycocine be derived from the bile poured out into the duodenum, or be formed elsewhere in the body." He then, coming to the question of the abnormal formation of uric acid in the human system, inculcates that:—"Just as in diabetes the essential fault lies in the inability of the system, either in the liver or elsewhere, to effect the metabolism of glucose which then passes into the circulation and is discharged by the kidneys, so in gout or gravel the imperfect metabolism of glycocine is the primary and essential defect. Unchanged it passes from the alimentary canal, or elsewhere, into the liver; there, under the action of the gland, it is conjugated with urea resulting from the metabolism of the other amido bodies, is converted into hydantoin, and then passes on to the kidneys to be combined there with another molecule of urea forming ammonium urate, a portion of which overflows into the circulation, and is converted into sodium urate."

It is easy, our author suggests, to understand how persons who are addicted to the pleasures of the table, and whose livers "are sluggish," suffer from the non-metabolism of the glycocine. But all such persons do not develop gout, neither is every gouty person a gourmand. We must therefore look upon some change in the nervous system, a change localized in the medulla or spinal cord, or both, as the most important factor in the etiology of the disorder, and as explaining the

hereditary tendency to excess of uric acid. Latham's work deserves careful study as on the borderland of something very new.

#### POST-NASAL CATARRH.

Dr. Woakes, in a new volume of the third edition of his work on "Deafness," published by H. K. Lewis, has introduced a most interesting and valuable physiological chapter on the correlating and reflex functions of the sympathetic system. In this essay, the author suggests that in man various regions of the spinal cord act as vaso-motor centres over limited areas. He also suggests that an excito-vaso-motor function exists between the afferent and efferent elements of a given sympathetic ganglion, and that these latter organs exercise a correlating function, in regard of such operations as belong to them, between tissue areas often widely separated. In subsequent pages the author illustrates this position from the inferior cervical ganglion, because his experience lies largely within the sphere of its operations; and because it being the controlling centre of the circulation of the labyrinth, the phenomena exhibited by alterations in the tension of the organ, whether extrinsic or intrinsic, direct or reflex, are attended with phenomena so striking and pronounced as to admit of no misinterpretation. The physiological chapter is followed in the body of the volume by essays on "Taking Cold," the "Catarrhally Predisposed," "Acute Catarrh," "Chronic Catarrh," "Hypertrophic Catarrh," "Follicular Pharyngitis," "Post-Nasal Vegetations," "Diseased Conditions of the 'Turbinated Bones,'" "The Treatment of Nasal Stenosis," and other allied subjects, all good and practical reading.

## RULES FOR NURSES UNDER TRAINING.

A little work by Dr. Blair, of Adelaide, comes to England, bearing its own recommendation in the value of its simple and excellent lessons. An address to nurses is followed by fifty-five short rules, which every attentive nurse might easily commit to memory, and which being followed, would make medical and surgical treatment much more certain, in the absence of the practitioner, than it now too often is. There is scarcely a single point which a nurse ought to attend to that is not recorded in this little work, and I hope some English publisher will persuade Dr. Blair to let it appear here as an original publication, purchaseable within the means of the important class of helpmeets in physic to whom it is addressed. The Ladies' Sanitary Association might very properly do another of its practical services by undertaking this task.

## POST-SCRIPTA.

Amongst other books and papers which have come in during the past quarter, and which I have only space to mention, I would like to name the following as deserving of special study:—

1. Dr. Wickham Legg's Bradshawe Lecture on *Cardiac Aneurisms* supplies an admirable practical text, followed by a bibliography of his subject, by far the most complete of anything of the kind, and very useful for reference.

2. Sir Joseph Fayrer's paper on *The Nature of Snake Poisons* is an essay bringing up the subject on which it treats to the latest date, and full of that wise and well-balanced judgment for which its author is so widely known. It will be laid down with pain, from the circumstance that so sound and careful an observer is



forced to declare that no cure for snake bite is yet known.

3. Sir Robert Rawlinson in three addresses on *The Social and National Influence of the Domiciliary Condition of the People*, discusses "houses as they have been, and houses as they are"; towns as they are; "old lessons in sanitary service and new lessons considered"; health; and various cognate topics, with the freedom of expression which a long life of observation richly justifies. To the young sanitarian these essays afford a model for his future work.

4. Dr. Aubrey Husband's *Sanitary Law*, a digest of the Sanitary Acts of England and Scotland, is a treatise of the most useful kind for medical officers of health, members of local boards, sanitary committees, sanitary inspectors, and all engaged in practical sanitation.

5. Mr. Mattieu William's *Shorthand for Everybody*, an essay of a few pages, presents an exceedingly simple description of a mode of learning to write in stenograph in a short time and with facility.

I cannot close the last page of the second number of the ASCLEPIAD without expressing how deeply I have been touched by the extreme kindness with which the first number has been accepted by the many cotemporaries at home and abroad who have noticed it. To produce each quarter a number single-handed is no labour with such reward.







*ON INTERMITTENT PULSE, AS A SIGN OF  
DISEASE, AND ITS TREATMENT.*

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**A**T the time of the publication of my first essay on intermittent pulse, in the year 1868, no medical writer whom I could discover had made the subject a special study. This is remarkable, because, if our present experience is that of the past, no condition of disease could have more frequently presented itself to the physician.

Since the appearance of my essay dealing specially with the subject, the phenomenon of intermittency has attracted much observation. It is not ignored now as it was by our predecessors, and it has not, I think, had any undue importance attached to it. This is the correct position.

The significance of intermittent pulse comes out most distinctly in cases where persons who suffer from it, being about to have their lives insured, undergo medical examination for the purpose of submitting to insurance regulations. To such persons the symptom is the more important because, from the peculiarity of their organization, they are most likely to present the symptom in

the worst degree, at the very same time when it tells most against them.

It is remarkable how very frequently at the life assurance board the question of intermittency is raised. I have often known it come up in two or three cases during one sitting, a fact which shows two things—first, that the affection must be very widely spread; and, secondly, that it is not so dangerous to life as might be expected from the peculiar character of the disablement.

It occurs to me day by day, as I go on watching the course of diseases, that intermittency of the pulse is a more common symptom than it formerly was. The reason for this is not obscure. The affection is due to nervous exhaustion of the vital nervous system, and in this day of emotional strain and excitement it must prevail in proportion to the causes of it.

Respecting the nature of the causes of intermittent pulse, I have dealt at such length in former essays, that I may be content to recapitulate them in this place. I take it that the affection is purely of nervous origin, and that it is always induced, where it is not inherited, by shocks upon the vital or sympathetic nervous system. I have traced it as arising from grief, from fear, from rage, from worry, from vital exhaustion; and in every instance which I have seen, it has been due to one or other of these influences. I have shown that while it may, by coincidence, be connected with organic disease of the heart, it has no necessary connection with such disease, but that it may exist when the structure of the heart is in the most perfect order, the muscular structure natural, the valvular machinery true and healthy, the blood in a condition free of any recognisable indication of disease.

It will be my object now to dwell: (a) on the significance of intermittent pulse in prognosis, *i.e.*, its significance as a phenomenon of disease; (b) and on the treatment which is most applicable for its relief, when by its intensity it calls for the interference of the physician.

#### THE SIGNIFICANCY OF INTERMITTENT PULSE.

In itself, when it is not present in an exaggerated degree, intermittency of the pulse is often less dangerous than it seems. It does not, as might be feared, carry with it the necessary idea of sudden dissolution from heart disease, for, as I have elsewhere shown, the heart is the regulator, not the prime mover, of the circulation. The harmlessness of the symptom in its moderate development is best shown by the facts of its common occurrence after middle age, and by the long duration of life in many of those who present it.

At the same time the symptom has its significance. Occurring in infancy, it is an important indication of the existence of serious nervous derangement. Occurring in young adults, it has the same meaning, and tells the story of commencing failure of power. Occurring suddenly after any great event, which has told upon the mind, it may be a sign of serious import. My own experience connects it now as the first physical indication of derangement in five cases of disorder of mind in which suicide was attempted, in two of the cases successfully. Further, it becomes an embarrassing sign in all conditions where there is diminished condensation of force in the nervous centres, where force is either insufficiently stored up or has been given out too freely.

In persons advanced in life, and in persons prematurely old, intermittency is often the herald of symptoms of nervous failure. In these examples the patient

has sometimes a singular preconception of impending danger; he is seized without reason with what I once heard a patient call "panic." These also are cases of very serious import. Again, the symptom may be increased until it lapses into veritable and fatal disorder, from continuance of morbid change in nervous matter.

In the large majority of patients there is an unconsciousness of the intermittency. We listen to the heart, we hear the phenomenon distinctly; we ask the patient, at the moment, whether he is conscious of feeling anything peculiar; he tells you he is not. In such instances, the intermittent phenomenon does not cover more than what would be one or at most two natural periods of cardiac contraction, and there is a long interval before the return of it. But when the intermittency covers a period equal to five normal strokes, or when it is repeated in shorter periods, several times in the minute, then the patient is painfully, often fearfully, conscious of the fact. Then breathing becomes irregular, then there is difficulty in keeping the recumbent posture, then there is sleepless agitation, terrible mental depression, a constant dread of death, sometimes with a singular longing for that event, and, finally, death itself, not suddenly, but by a lingering and sinking asthenia. These are true cases of what has been poetically called "broken heart."

I have seen one well-marked case, in which it was impossible to attribute death to any other cause than intermittent cardiac action, and I do not remember any case where the symptoms, which long preceded death, were more acutely painful. The heart intermitted in this patient, ten, and even twenty times in the minute, and some intervals of hesitation were so prolonged, that sense of faintness and impending death tormented the

sufferer. He feared to lie down, since that increased the evil; he feared to sleep for the same reason; and as he got weaker from pain and broken rest, the intermittency itself became intensified, despite all our efforts, until death brought the release.

A case such as is here related is, truly, extreme and rare, and I do not adduce it as suggesting undue alarm respecting intermittent action. I would give to the symptom its just value, and no more. Whenever it is persistently present in any person, the actual value of life as compared, *cæteris paribus*, with the life of another person who has no such symptom, is reduced; the power for work is less; the power to meet extremes of heat and cold is less; and the power to meet the anxieties and calamities of life is, unquestionably, much less. The man or woman with a hesitating heart is thereby unfitted for sudden tasks, demands, resolves, which, when the heart is firm, are considered as of comparatively little moment; for when the heart hesitates, the brain, which reposes for its power on the blood the heart supplies to it, falters with the heart, just as the gas flickers when the steady pressure is taken off the main. From these circumstances some persons who once were known as resolute and determined, lose those qualities when they are subjected to intermittent action of the heart, becoming, as their friends say, uncertain and doubtful in character; becoming, as they themselves feel and know, less the masters of themselves, and less secure in their own work, and skill, and power.

Another point is worthy of note. Persons in whom there is permanent intermittent action of the pulse pass through all acute diseases with less chance of recovery than others of similar age and like constitution who have no such failure. They sink more readily from



surgical operations, from falls and injuries, from influenza, from acute congestion of the lungs, from inflammatory attacks, and particularly from typhus and typhoid fevers. I would look upon a man's chance of recovery from typhoid if he were fifty years of age, and had a steady heart, as preferable to that of another man at forty, in whom intermittent action of the heart was developed before the occurrence of the disease, or in whom the symptom came on, as it sometimes does come on, in the course of the disease.

Such are the principal facts conveyed to the mind when the phenomenon of intermittency of the pulse is before the practitioner. The phenomenon is truly to be reckoned only as one symptom, and I would in no sense exaggerate its importance. At the same time, in severe justice, it is correct to say of it, that the person who presents it, though he may live long, is of infirm body; that the mechanism of his heart and circulation is out of tune; that if ever his heart should be called upon for a great effort, it will not be prepared for the effort; and that if ever, from mental strain or pressure, his heart is weakened, it will succumb more easily than it ought, naturally, to the resistance put upon it.

Thus the phenomenon, symptom though it be, is of moment. It is of such moment that if two men, of equal age, build, education, and power, were put into any contest, mental or physical, and the one had, and the other had not, intermittent pulse, the chance of success would be altogether in favour of the man whose pulse was not intermittent. In like manner if two men, of equal apparent build, had to contend with a surgical operation, a disease, or a given equal amount of anxiety, the chances of resistance would be very largely in favour of the man whose heart was not intermittent.



The phenomenon has, therefore, its general meaning, both for the physician and the surgeon. It has also its particular meaning, since it may be raised into fatal consequence by disorders which need not, in its absence, prove fatal.

THE PERSISTENCY OF INTERMITTENT PULSE.

In children, the symptom of intermittency of the pulse may pass away with growth and increase of strength ; in adults, when the symptom is once established, it never, I believe, goes away entirely. It may be absent for long periods when the general health is good, but it returns on every occasion of depression of power, and is very easily re-induced by agencies which act deleteriously on the nervous system. Excessive venereal gratification, excessive smoking, deficiency of sleep, or dissipation, act powerfully in increasing the evil. In persons at or past middle age, the symptom, if it once be fully developed, continues persistently, and often to extreme old age. One of my patients, who died at eighty-six years of age, told me he had been discovered to have an intermittent pulse when he was forty-two, and that he had never failed to exhibit the phenomenon since that time.

I have noticed often the hereditary character of the phenomenon. On the fact of heredity, there can, I think, be no doubt. I have once seen an infant who in the first hour of his life was discovered to have an intermittent pulse ; and now, although he has reached the age of twenty-five years, he shows the intermittency when under great strain, or under influence of depression.

In this case the hereditary tendency came down on both the paternal and maternal sides, and will probably never be lost.

## TREATMENT—POINTS OF PRACTICE.

There is no known specific treatment for intermittent pulse, but, whenever the symptom of intermittency is present, there are certain general lines of treatment which should always be enforced by the physician.

## I.

In the case of young children, when the intermittency is clear, however infrequent it may be, the utmost care should be taken to avoid every source of mental emotional excitement. A child having intermittent pulse should not, under any pretence, be oppressed with study. He should not be subjected to any amusements which powerfully excite the mind; he should not at any time be exhausted by physical fatigue; he should be well fed, warmly clothed from head to foot, and, above all things, should be allowed to have abundant sleep. Ten to twelve hours' sleep is not a moment too much. Moreover, such a child should never be put to sleep with stories which excite dreams or cause alarm.

## II.

In adults, equal care should be taken, and, above all things, attempts should be made to remove impressions derived from any untoward event. Change of scene should be recommended, while a carefully regulated diet, abstinence from exhausting pleasures and abstinence from exhausting labour, especially mental labour of any one particular kind, should be encouraged. Good sleep is here again the most valuable of remedies. Eight hours of sleep out of the twenty-four are essential, nine hours are still better.

Two other special points of advice are of moment. It not unfrequently happens that, by accident or by direct

information, patients learn the fact that their pulse intermits. Then they begin to feel their own pulse, and become charged with dread of sudden death. As the disorder is of itself mental, this watchfulness and fear will increase the frequency of the intermittency. With these patients, a word from the physician, timely and firmly spoken, is often the best prescription. He assures them, on the results of experience, that their malady is not of necessity fatal; he recommends them not to enquire after the symptom, and if he can succeed in persuading them to his views, which he may honestly try to do with all his influence, he will effect the most marked improvement in their condition. Again, it sometimes happens that patients conscious of the failure of the heart resort to alcoholic stimulants as a means of relief. For a moment, by its exalting the activity of the heart, alcohol affords relief, but the depression that follows calls the more rapidly for a return to the supposed remedy, and a fictitious benefit leads to a habit which excites structural changes and hastens death.

### III.

In cases of sudden intermittency, with symptoms of cerebral congestion, depletive measures are sound. A purgative is essential, and blistering at the back of the neck is always useful. I have seen also great advantage in these cases from dry cupping and even from abstraction of a moderate quantity of blood by the cupping glasses.

### IV.

In extreme forms of chronic intermittency of the pulse it becomes often imperatively necessary to subdue nervous excitement, and to induce rest. For this latter purpose, opium is the sheet anchor. It must

be given freely when it is given, and not too frequently. Small and repeated doses of opium excite, depress, and give no rest. A full dose, equivalent to a grain or even two grains, produces, on the contrary, no excitement, but gives sound sleep and that quietude of circulation which is essential to secure a satisfactory relief. I have sometimes, where there was much depression, combined opium with full doses of quinine, and with marked benefit.

## V.

Concerning aged people who suffer from what may be called chronic intermittency without consciousness of the symptoms, no special rule requires to be laid down. They are themselves usually too tired of the excitements of life to care for them, and if they are not, then the observance of the general principles applicable to children and adults extends equally to them.

### *Some Additional Points of Practice.*

The above general rules of practice, written in 1868, hold good, and require no modification; but I have since learned a few details of practice which are useful, and which deserve to be added to this essay. In the first place, whenever with intermittency of the pulse there is anæmia with inactive condition of the bowels, and distention of the stomach and intestines with gas, it is very good practice to add, to the general rules of treatment, "a tonic" so called, and of all tonics Easton's Syrup of the Superphosphate of Quinine, Iron, and Strychnine, is one of the best. This syrup, which contains the thirty-second part of a grain of strychnine in a fluid drachm, should be administered in doses of a drachm three times daily, a little time after food, and the patient should be induced to look upon the remedy in the light

of a food rather than a medicine. The syrup, under the careful observation of the practitioner, may be continued for two or three months at a time without danger.

When there is much restlessness with the intermittency, as well as want of power, I am accustomed to administer one or other of the Bromide syrups, which I once introduced to the profession in the pages of the *Practitioner*, during the late Dr. Anstie's editorship. The syrups of the Bromide of Iron, Quinine, and Strychnine, or of the Bromide of Iron and Strychnine, or of Quinine and Strychnine without the Iron, replace Easton's preparation very effectively. The dose of these syrups is one fluid drachm.

In some cases of intermittent pulse I find a tendency to periodical neuralgia. Sometimes the neuralgia attacks the nerves of the face, sometimes the sciatic nerves, sometimes the pneumogastrics. When this symptom is present, the intermittent action is intensified; not, I believe, from any direct connection between the neuralgic pathological state and the irregular action of the heart, but from the broken rest and anxiety which the neuralgic pain induces. In these cases, in addition to the above-named syrups, which still maintain their position, quinine in free doses—two to five grains—is required, together with morphia or codeia, when it is necessary to procure sleep artificially. Hydrate of chloral is less useful in this class of cases than morphia or codeia. I have sometimes combined the hydrate with morphia, but as the advantage has not seemed very striking, I order the combination rarely now.

There are classes of cases in which intermittent pulse is connected with great general prostration and premature breaking up of the body; cases in which there is some organic disease, such as chronic bronchitis, em-



physema, senile phthisis, chronic degeneration of the kidney, or other organic change; or some general systemic disorder, such as diabetes or cancer. In any of these cases the intermittent action is a terrible addition to the distress of the sufferer, and it may require to be treated itself as the worst present evil. Under these circumstances I find the following compound—with an occasional alterative—of most service for the intermittency:—

*Mixture of Iron, Morphia, and Ammonia.*

Sulphate of iron, grs. xxx.

Carbonate of potash, grs. xxv.

Rub together dry into a fine powder; pour on distilled water in small quantity so as to make a thin paste, and when the green carbonate of iron is formed, add—

Bicarbonate of ammonia, grs. xxiv.

Solution of hydrochlorate of morphia, 5 ss.

Finally add—

Pure alcohol, ℥j.

Pure glycerine to ℥ij. Mix.

Of this mixture the dose is from two to three fluid drachms, which dose may be taken three or four times in the twenty-four hours. After sixteen years of close observation, I know of nothing that gives such speedy relief to the intermittent action, or that more strictly acts in the sense of a “tonic” than this mixture, in the cases named.

I have made many inquiries in order to ascertain if there be any one particular remedy which so influences the nervous mechanism of the heart as to exert an immediate controlling effect over intermittent action. The result of my research is that there is only one agent which can be said positively to influence it effectively; I mean to influence it *at once* in such determinate manner that an effect is seen to follow upon a cause.

The agent to which I refer is alcohol, and the mention of alcohol brings up the whole question of its use in cases of intermittent action.

It has long been known that the direct physiological action of alcohol is to quicken the motion of the heart, and Dr. Parkes and Count Wollowicz, in an admirable paper published in the proceedings of the Royal Society for May 1870, have demonstrated that in a healthy man the daily increase of the beats of the heart under 4·8 oz. of alcohol, as compared with the number of beats when water is the only beverage, is rather more than thirteen per cent.

In intermittent pulse this direct action of alcohol on the heart is shown with singular effect. I have seen in an extreme case, where the fact of intermittency was recorded ten times in the minute at least, a total cessation of the phenomenon within five minutes after the administration of six fluid drachms of pure alcohol in water, the circulation at the same time being rendered more rapid. This action of alcohol is so decisive that the patient himself soon becomes conscious of it, and unfortunately resorts sometimes to the remedy, to his ultimate disadvantage.

In my original essay on intermittent pulse, I wrote the substance of what is contained immediately above ; and although I have since changed my views respecting alcohol as a beverage, and have for ten years abstained from it, I repeat that nothing relieves the intermittent action so rapidly as alcohol judiciously administered, and that nothing clenches the affection more decisively than alcohol indiscriminately prescribed.

It is a duty for me now to explain, after a longer research on the action of alcohol than I had made when I first wrote on intermittent pulse, what seems

to me to be the correct handling of the subject at the present moment.

The good which alcohol effects is due to the power it possesses of reducing the arterial tension. It takes off resistance from the heart, and thereby the heart works easier. But just for the very reason that it reduces arterial tension it tends to produce unnatural nutritive changes and to exhaust nervous power, so that if it be often repeated, it does the very thing which ought not to be done.

The question therefore arises—To what extent is alcohol useful as a remedy for intermittent heart? This is a very delicate and difficult question to answer; for as the remedy itself, if carried too far, increases, after a time, the primitive evil, and as it is a remedy always at hand and viciously pleasant, its bad rather than its good influence is the most probable event.

The advice I have to offer on the matter is nevertheless simple and straight. I recommend that all who suffer from intermittent pulse should abstain from alcohol in every shape as far as is possible, and resolutely abstain from every alcoholic fluid respecting the character of which there is the merest doubt. There are some fluids, such as champagnes, sweet ports, and even light acid wines,—clarets, burgundies, hocks,—which, so soon as the short stimulating action they produce has passed away, induce great prostration, with marked increase of intermittent action, if they have been taken freely. All these must be avoided, together with brandy, whisky, rum, and gin.

Contrary to what I originally taught, I now recommend that the sufferer from intermittent pulse should abstain from every alcoholic drink known as a common

drink or beverage. At the same time I would not withhold the occasional use of alcohol in extreme conditions, or what may be called emergencies. If after great fatigue or excitement or anxiety, there is sleeplessness, restlessness, and painful knowledge, on the part of the patient, of the hesitation of the circulation, half a fluid ounce to six fluid drachms of pure alcohol, mixed with warm water, will act, generally, in the most effective manner. It will bring rest, and often sleep. But it must be repeated only after an interval of seven or eight hours; if it be carried to the extent of producing the third, paralysing or narcotic, degree of alcoholic stimulation it will have conferred evil instead of good.

In the medicinal sense it is best to prescribe alcohol of sp. gr. 0.830. Half a fluid ounce to six fluid drachms will be sufficient in four ounces of water, to which may be added ten grains of bicarbonate of potash or twenty minims of sal volatile if there be acidity of stomach or eructation.

In some instances, instead of prescribing common or ethylic alcohol, I substitute pure methylic alcohol. This is a much lighter spirit, and is eliminated more quickly from the body. The dose is the same as for common alcohol, and may be prescribed in precisely the same way, with the advantage that it may be more frequently repeated than the same dose of ethylic alcohol.

It will be seen from these observations that I recommend the use of alcohol purely as a medicine. I do so most earnestly, under the conviction that whenever it is more than a medicine, it is far more harmful than useful.

I think I have now said all that needs be said

respecting the use of alcohol for intermittent pulse. I have expressed a fact, a practice, and a caution. The fact, that alcohol by its action on the nervous mechanism will temporarily remove intermittent action. The practice, that in extreme cases and at extreme crises, alcohol may be advantageously prescribed to relieve the intermittency. The caution, that in prescribing alcohol it is never necessary to let the occasional glide into the habitual practice of taking it, the tendency of the remedy being, when it is often and systematically repeated, to increase the primitive evil.

It is better that a patient who suffers from intermittent pulse should not smoke tobacco. It has been assumed by some that tobacco may even induce intermittent action, and I have seen one or two cases which would engender a suspicion that both smoking and snuff-taking might be set down as causes. But longer observation tells me the suspicion is not correct. If the practices named were true and independent causes of the phenomenon, we should find, frequently, cases in the male sex where the cause stood alone, and we should also find the phenomenon occur much oftener in men than in women. In truth, however, I have never seen a *pure* case of intermittency from tobacco; neither have I been able to discover that the male sex is specially liable to the affection. At the same time, it is unquestionably true that, when the symptom is developed, smoking and snuffing intensify it, not, I think, from any special influence, but from the general debility induced, the steady indigestion which the narcotic sustains, and the restless muscular action—tremor—which it favours and supports. Tobacco, in a word, acts like other depressing



agencies, such as loss of blood, or want of sleep, or deficient food, or bad assimilation of food. It injures as a secondary cause; it does not primarily produce the affection.

In men who chew tobacco, and in men and animals under the direct poisonous influence of nicotine, I have watched for the symptom of intermittency of the pulse without result. The action of the heart may be feeble and irregular, the muscles in a state of restless tremor, and the body cold; but there is no pure intermittent action. The symptoms from nicotine are those of cardiac apnœa, of failure of the right rather than of the left side of the heart, with difficult respiration; and, in man, spasmodic pain passing through the thorax, from the sternum to the crura of the diaphragm. A marked case of this kind is recorded in the essay on cardiac apnœa in the first volume of the ASCLEPIAD, published in 1861.

The remarks made in reference to tobacco apply equally to tea. Tea does not produce intermittent pulse, but when the symptom is present it very seriously increases it; tea, in short, is an article of diet which in all cases of irregular action of the heart should be scrupulously avoided. Coffee is less objectionable.

In respect to the diet of persons suffering from intermittent pulse, little in the way of special rule can be said, because in each case there is commonly some other functional organic disorder which calls for special attention, the intermittency remaining as a systemic failure rather than as a distinct malady. But one general remark applies to the general condition—viz., that food should be taken by sufferers from intermittent action in moderate quantities and *frequently*.

Long fasting is unspeakably prejudicial, and makes itself speedily felt by the patient, who suffers, while fasting too long, from an indescribable exhaustion, which is not hunger and not faintness, in the natural sense of those terms, but a strange mixture of both sensations with a frequently recurring impression that if food do not immediately come death must. The nervous supply of the heart in these cases is sensitive to the least failure of power, and requires renewal every three or four hours during the working day. At the same time, the system rebels against a large, oppressive meal, difficult of digestion.

One note more respecting meals. There is a common feeling against *late* meals, late dinners or suppers, and I have no intention of opposing that which is not only a common, but, as a rule, a natural and correct objection. Experience, however, tells me that to retire to rest with the body fasting is as bad a practice as to retire to rest with the stomach busily digesting. A light meal taken an hour or two before sleep is, I think, good for every one, and it is essential for those who suffer from intermittent pulse. The last meal should, however, not be large, and should consist of some very simple easily digested food, such as milk, or cocoa, with bread and butter, or some similar food, tea being specially excluded.

In extreme cases of intermittent pulse it is good practice to let the patient have a glass of milk, or of milk diluted with water, at the bedside during the night. The period of the approach of early morning, the hours between two o'clock and the dawn of day, is a period when the strongest are at their weakest, and the weakest at their worst; and so it commonly happens that the very weak die in the

hours named. In the same hours persons who suffer from feeble and intermittent action of the heart commonly become restless, wake with sensation of exhaustion, and, if they are not sustained, sleep afterwards indifferently. At this period the reserve of food I have suggested is the best remedy; it relieves the exhaustion and restlessness, and ensures return to a sleep that will be undisturbed by melancholy dreaming and will yield refreshment.

A word as to warmth and clothing. Persons who suffer from intermittent pulse, whether they be young or old, should at no time expose themselves to loss of bodily heat. They should not, for example, take cold plunge baths nor cold shower baths, but should in every habit study to follow a medium course. In every season the body should be so clothed that sensation of coldness should not be felt. The bedroom should be kept all the night through at a temperature not lower, at any time, than 60° Fahr. Care should be taken, also, to protect the body from damp and wet. In a word, every such provision should be made as will sustain equality of the vital processes, so that the nervously disabled heart may neither wait long for new support nor be overtaxed with work.

## *THE FOLLY OF SCEPTICISM IN REMEDIAL ART.*

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**R**EMEDIAL medicine is a science: remedial medicine is an art. From failing to recognize these truths there is a tendency in our days to allow the art to stand aside as almost meaningless, and to force on the science to the exclusion of the art, or, at best, to the acceptance of it as an undesirable necessity. The scepticism which has thus been so strangely and singularly developed in the modern objective school of medicine, has sprung out of a knowledge of the fact that many diseases to which the body is subjected will, under favourable circumstances, pass away without the administration of any remedy whatever. Scepticism is not science. It is as far removed from science as the errors which it perceives and which it timorously avoids. We may accept, indeed, as one of the most certain of facts, that a sceptic is a failure. The very existence of the sceptical mind implies an uncertain feebleness of mind in which the over-sensitive judgment, in conflict with intellectual development, makes traitorous doubt supreme.

So it happens that objective sceptics are not satisfied with themselves, while they utterly fail to implant a

true satisfaction in others. To the sick man, who is first to be thought of, sceptics are, as they ought to be, unsatisfactory to the last degree. They can never hold ground on which to institute a progressive and accepted school. In their hands medicine, as a practical art, would cease to exist. With all this the sceptics have a certain measure of wavering reason on their side. It is true, as they see, that some diseases pass away as easily without treatment as with it; which fact leads to the generalisation that those diseases, in all phases, would pass away equally well without as with the so-called resources of remedial art. It is in this that the sceptics find the errors which startle them, because even on their plan they are obliged to recognize a certain measure of mortality. Their patients sometimes die, and thus solemnly advised that their expectant plan may fail them, they are more sensible of their failure than are they who, in ignorance, have at any rate done something, and that something the best they know.

Nevertheless, what the sceptics have discovered is, or may be, positive knowledge, as many practical illustrations will serve to show.

Let us take the example of the acute febrile diseases. A number of these diseases are apt to subside without any medical interference. After a brief interval, in which the animal combustion has been excited above what is natural, and a new order of physiological changes has been set up, the balance swings round again to natural equilibrium, and the symptoms that made up the disease disappear. "See then," says the sceptic, "how definite is the recovery. Why should I put in my ignorant skill to interfere? What could I do that is better than what has been done without me?"



From positive remedial art the answer would come clear enough. It would say, you could do nothing better; because in this case the natural equilibrium was restored within the limit of danger. If you, from hour to hour, had watched the animal combustion in that favourable case, you would have discovered that the temperature did at no time exceed what is within the range of safety. Before the fatally dangerous increment of fever was reached, the ebullition was over, the danger from secondary changes was over, and there was no real necessity to meddle with the cure that was in progress. "But," adds the positive scholar, "if, in the course of the increased combustion, the dangerous increment of heat had been reached, then the natural process, if you like to call it so, would be as certainly towards death as a fall of heat would be towards recovery, and here, therefore, I want to be ready to come in at the right time, and by my positive action bring back the erring function to its natural limits. A fire in the room of a house may, truly, be extinguished by its own products of combustion, and may cure itself, but if it does not cure itself, if it begin to rage, then the means of external cure are demanded—the engine and the water."

The school of placebo, the sceptical school of medicine, ranks, or rather runs side by side with, every hesitating schismatic school. It is admirably fitted for fashionable practice, in which many of the diseases, as well as the persons affected by them, are dilettanti. For the stern realities of professional labour it is mere fancy practice,—good, sometimes, for making the fortunes of its delicate professors; unworthy the consideration of the thoughtful, the steady, the earnest student of natural law and minister of health. The

true student, the true practitioner formed out of the student, may accept the sceptic's nervous doubtings, and trust them to the full extent of their soundness. They will always lead a very little way, but while they lead they need not be despised.

The true student may accept the science of prevention in all its fulness; he may make it his glory to improve it, to perfect it. He may with equal zeal improve and perfect the art, in full confidence that with all his preventive science the supplementary art will, times and often, be called for. The engineer with his hand on the lever of the engine knows at what pace he may let his engine drive, how long it will drive safely, and how soon it will cease to drive by the going out of the force that drives it. But this is no reason why he should not by his skill stop the engine if it be exceeding in its motion the limit of safety, or increase its force if he wishes it to do more work. The same rule applies to medical art. Where natural safety or power ends art should interpose, and by fixed rule do its best to determine and govern the result.

The failure of art, so far, depends on the absence of fixed rules in promoting it and the strength, or, more correctly, the excuse, of scepticism is the result of this deficiency. The sceptic may wait for ever to see the natural subsidence of phthisis pulmonalis, of cancer, of hydrophobia, and of a long train of other mortal maladies. But until art is placed on positive rule and precept, clearly defined and rationally understood, the folly of scepticism will still find place, and, doing nothing itself, will make progress very hard.

The hope is that one day some great vital truth will break upon us, and show art an easy passage towards victory over many diseases now called fatal under any

treatment, diseases in which sceptical expectancy can never be of the least avail. In detail, we see, indeed, the triumphs of art. What sceptical method would ever have re-opened the eyes of those who have become blind from cataract? What would the choicest and most refined scepticism have effected for the removal of stone in the bladder, or for the radical cure of ovarian dropsy? These are grand triumphs, but they are not the grandest. That which has to come is an art, derived from science, which shall place the living body that is not physically crushed or physically degenerated in its vital parts, in conditions under which it cannot die; so that when life is threatened by the mere temporary failure of one organ of the body, art shall come in, and supplementing the failure, maintain the life until the natural function is restored.

In such a day of victory of art, combined with preventive science, death would only occur from extreme accidents and natural decay. There will "be no more thence an infant of days nor an old man who hath not filled his days; for the child shall die a hundred years old."

I, for my part, venture to predict that such a day is near at hand.





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**DR WILLIAM GILBERT,**

Physician to Q<sup>ueen</sup> Elizabeth.

*From an Original Picture in the Bodleian Library Oxford.*

*Publ. May 1866 by S. R. D. Juno Coll. 24. 31*



THE FIRST ELECTRICIAN,—WILLIAM  
GILBERT, M.D.

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IF there be one old town in England more interesting than another to the historical scholar it is Colchester. Every class of scholar may find some interest there, and we of medicine need not be behind the rest. In Colchester we shall find the birthplace and the final resting-place of the illustrious pioneer in science, who gave us the word electricity, and the first true idea of the power. The pioneer in question was William Gilbert, M.D., friend and favourite physician of Queen Elizabeth.

The best account of this man is supplied in *The History and Antiquities of the most Ancient Town and Borough of Colchester*, by Philip Morant, M.A. MDCCXLVIII.

Gilbert wrote his own name Gilberd. He was the son of Hieron (Jerome) Gylberd, gentleman, a native of Hintlesham in Suffolk, but afterwards a burgess of Colchester; made so in 1528. William was born in the year 1540. He studied both in Oxford and Cambridge, and afterwards travelled on the Continent, where

(according to Morant) he had conferred upon him the degree of Doctor of Physic. Dr. Munk, however, has gathered from Mr. Cooper, the learned author of the *Athenæ Cantabrigienses*, that he was of St. John's College, Cambridge; that he proceeded B.A. 1560; was elected fellow of St. John's, 21st March, 1560-61; M.A. 1564; M.D. 1569; and senior fellow of his college 21st December, 1569.

Gilbert settled in London when he was thirty-three years of age, and commenced practice as a physician. He became a Fellow of the Royal College of Physicians, passed through the office of Censor and Elect, and in the year 1600 was made President.

That he practised his profession with great success is clear from the evidence of all who have written concerning him. He was received, says Morant, with the highest favour by Queen Elizabeth, whom he served as chief physician, and from whom he received a legacy, the only legacy she left to any one. He also, for the short period between the Queen's death and his own, acted as physician-in-chief to James I. By his will he left all his books, globes, and cabinet of minerals, to the College of Physicians.

Morant states that there was a portrait of Gilbert in the Schools Gallery at Oxford in his time, which portrait showed him to have been tall of stature and of cheerful countenance. It is from this portrait that the plate opposite the preceding page was probably taken, although we have now no direct evidence of the fact. The plate has been kindly lent to me for autotype copy by permission of the Treasurer of the Royal College of Physicians.

From this plate as the centre of the subject Mr. Arthur Ackland Hunt produced, a few years ago, a very

fine historical painting, representing Gilbert making an experimental demonstration in electricity, before Queen Elizabeth. In the picture Sir Walter Raleigh, Drake, and Burleigh contrast admirably with the ladies of the Court, who, if I remember rightly, are more interested in a young courtier than in the experiment.

Gilbert's house in Colchester was anciently called "Tymperley's" or Tympornell's. In Morant's time it "was the same as Serjeant Price, late Recorder, lived in, and still possessed by his widow and relict, Bridget Price."

Where Gilbert resided while in London we have no record; but that he had every facility for his own line of research there cannot be reasonable doubt. To that research, so remarkable for its originality, and so important in its future bearings, I now direct, briefly, the attention of the reader.

The peculiarity of the work which first strikes us lies in a word. Gilbert is the man who gave to us the word *electricitas*. It had been known, long before his time, that the rubbing of amber (electron) caused amber to attract some light bodies. Gilbert, whose great line of investigation was on the magnet, differentiated between the attraction exerted by the magnet and that exerted by the excited amber. He connected also the kind of attraction evidenced through amber with that produced by exciting glass, wax, jet, and other substances since known as electrics. His great work "*De Magnete*," published in 1600, becomes thus the basis of all our electrical science from that date. Galileo, Bacon, Thomson the historian of the Royal Society, Priestley, and in our later time Hallam, all bear witness to the solid quality and originality of the *De Magnete*.

For preparing this short memoir I have, with the kind assistance of my friend Mr. James Menzies, made a study of this book, some portions of which are sure to be of interest to all scientific and medical readers.

*De Magnete* opens with a preface to the candid reader and student of magnetic philosophy, a preface so good that it admits, scarcely, of curtailment. In it Gilbert pours forth the spirit of true philosophy. In it he shows that he was well acquainted with and a master of the experimental method of research in natural science. In it he demonstrates that he was actually carrying out, in practice, that which the author of the *Novum Organon*,—who, upon natural things, wrote, as Harvey said, “like a Lord Chancellor”—was putting on paper as a series of speculative disquisitions and projects.

Thus in the strictest sense of the term Gilbert might be called the father of modern experimental science.

With Mr. Menzies’ kind and learned aid I give this remarkable preface in full, for the first time, I believe, in English dress.

PREFACE TO THE CANDID READER AND STUDENT OF  
MAGNETIC PHILOSOPHY.

“Since in the discovery of the secrets, and in the enquiry into the hidden causes of things, stronger proofs can be obtained from reliable experiments and demonstrated arguments than from the probable conjectures and opinions of the ordinary professors of philosophy; since, therefore, the famous substance of that great magnet, our common mother the earth, hitherto wholly unknown, and the conspicuous and eminent forces of this globe of ours, may be the better understood, I propose to begin from the common magnetic, earthy, and iron bodies, those substances of our

globe which we can grasp with our hands and perceive with our senses, and then proceed to demonstrable magnetic experiments, and so penetrate, for the first time, into the innermost parts and secrets of the earth. For after very many of those things which have been obtained from the heights of mountains, the depths of seas, the profoundest caverns and buried mines have been seen and understood, then at last we shall have learnt the true substance of the earth. To the investigation of magnetic force (such, indeed, are the wonderful and surpassing qualities of the bodies known to us) have I given a great and prolonged attention. Nor have I found this labour idle and unfruitful, inasmuch as during the progress of my daily experiments, new and unexpected properties have revealed themselves, and my love of knowledge hath so much increased from the diligent survey of things, that I have essayed to show forth the very interior of the terrene globe and its true substance on magnetic principles; to reveal unto men the Earth (our common mother), and to point it out as with the finger, by true demonstrations, and by experiments manifest to the senses.

“And as geometry ascends from sundry very simple and easy principles to very high and difficult conceptions, by which the wit of man climbs above the firmament, so our teaching of magnetic science first sets forth in convenient order certain obvious facts; out of these more remarkable ones come to light; and at last, in sure order, the concealed and greatest secrets of our globe are opened, and the causes of those things known which, either through the ignorance of ancient, or the neglect of recent times, have been overlooked and unknown. But why should I, in so vast an ocean of books, by which the minds of ingenious men are



troubled and fatigued, through which very silly productions the herd of unreasoning men are intoxicated, rave, are puffed up, and create literary broils, and while declaring themselves to be philosophers, physicians, mathematicians, and astrologers, neglect and despise men of learning: why should I, I say, add anything new to this republic of letters, and expose this philosophy, glorious, seemingly new and incredible, on account of so many things hitherto unrevealed, to be condemned and torn to pieces by the ill words of such persons as are pledged to the opinions of other men, or are the foolish corrupters of good arts, bookish laymen, grammarians, sophists, wranglers, and froward individuals of the meanest kind? But unto you almost alone, you honest students of philosophy, who seek knowledge not from books only, but from things themselves, I commend these principles of magnetism, set forth in a new mode of philosophising.

“And should it not seem fit to some to assent to sundry opinions and incredible statements, they may nevertheless contemplate a great array of experiments and discoveries (such as all philosophy ever flourisheth in), which have been searched out and demonstrated by me with much care, watchfulness, and cost. In them rejoice, and to good purpose enjoy them, if ye be able. I know how difficult it is to give freshness to old things, brilliancy to the antiquated, light to the dark, grace to the despised, credibility to the doubtful; how much more difficult is it to obtain and establish some authority for things new and unheard of, and which are opposed to all the beliefs of men. Nor for that, however, do I care; since I think it is to few that the philosophic spirit hath been vouchsafed. Whoever desires to make trial of the same experiments, let

him handle the substances not unskilfully and carelessly, but wisely, aptly, and in a business-like way; and let him not (when he fails of success) begin to find fault with my inventions, for nothing hath been set forth in this book which hath not been examined and many times tried and repeated by me. Many things in the reasonings and hypotheses will, perchance, at first sight seem very hard to be received, yet I doubt not but that at length they will obtain authority from the clear proofs given. Wherefore, in magnetic science, they who advance furthest, trust most in, and receive most profit from, the arguments adduced; nor in this science will anything easily become clear to any one in which all or nearly all points are not known. Almost the whole of this department of natural science is new and unheard of, except what a few writers have delivered concerning certain familiar properties of the magnet. Wherefore, I but little approve of the aid obtained from the ancient Greek writers, because neither the poor Greek arguments nor the Greek words avail to demonstrate or cast any light upon the truth. For our magnetic teaching is at variance with most of their principles and received opinions. Neither have I brought to this work any craft of eloquence or adornment of words, but this only have I done that things difficult and unknown may be so handled by me and set forth in that style of writing and in those words absolutely necessary, that they may be clearly understood. I sometimes, therefore, use certain new and unusual words, not that by means of the childish veil of words, shades and darkness should be cast upon facts (as the alchemists are wont to do), but that hidden things, which have no name, and which have never before been understood, may be plainly and clearly delivered by me.

“After describing our magnetic experiments, and making known the like elements of the earth, I proceed to the general nature of the whole globe itself, wherein it is allowed us to philosophise freely and with the same liberty which the Egyptians, Greeks, and Romans formerly used in publishing their opinions: whereof very many errors have been handed down to later authors, and in which smatterers still persist, and so wander, as it were, in perpetual darkness. To each of these ancient and earliest fathers of philosophy, Aristotle, Theophrastus, Ptolemaeus, Hippocrates, and Galen, let due honour be paid: such are the writers who have diffused knowledge to subsequent ages; but our time hath disclosed and brought to light very many facts which they also, had they been alive, would doubtless have accepted. Wherefore we hesitate not to deliver, in an argument founded on truth, those things which have been discovered by us after prolonged experiment.”

## THE BOOK.

Such is the preface to this wonderful book. Its first chapter supplies a description of the magnet and of magnetic virtues. In its second chapter it introduces us to the word and the power, electricity. It explains that not only will the magnet attract some bodies brought near to it, but some other things will do the same, after they have been excited by friction. Amber, *electron*, is one of the substances which can be thus excited; jet is another of the same kind; but amber came first into knowledge as evincing this remarkable property.

“Amber,” Gilbert says, “holds flies, ants, and other small creatures shining in eternal sepulchres”—*aeternis*

*sepulchris relucentes.* There was a touch of poetry in William Gilbert.

The bodies of this nature may, then, be called electrics, and the property they present may be called the electric, or electrical. There are many bodies which act as electrics, besides amber and jet. The diamond, the sapphire and some other precious stones, glass, sulphur, mastic, sealing-wax and rosin, are electrics.

Gilbert seems to have discovered that glass, sulphur, and wax are electrics, his mode of discovery being as simple as it was effective. Small strips of metal suspended from their centres were the best objects. To them the excited electric was presented, and the attraction was the proof of the electrical action.

He was very exact in noting the conditions under which electrical action could be best observed. He found that it is always most active when the air is dry and when the wind is north or north-easterly. Moisture destroys the effect, and even the presence of the breath is sufficient to impair it. He made an attempt to electrify water, and by presenting an electrified body to drops of water, produced a conical shape in the round drop. He discovered that if an electrified rod were brought near to the dense smoke rising from a burning substance, the smoke was attracted. He suggested that electrical attraction was due to the electrical fluid or effluvium rushing from one substance to another of similar kind, and cohering, as two drops of water cohere and run into one. He suggested also, on this matter, a distinction between magnetism and electricity.

“The difference between magnetic and electric force is, that all magnetic bodies attract by their mutual strength, while in electric bodies the electric attracts only; the body attracted is not changed by its natural

force, but is drawn spontaneously by the nature of the material of which it is composed. Bodies are drawn towards electric bodies in a straight line towards the centre of electricity; but the magnet attracts the magnet only at the poles directly, at other points obliquely and transversely, even as they adhere and hang together. Electric motion is the motion of an accumulation of matter; magnetism, of arrangement

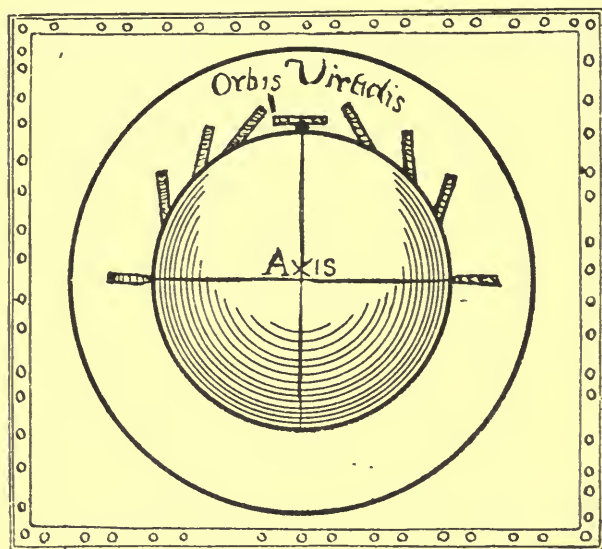


Fig. 1.

and order. The globe of the earth is collected, and coheres by electricity. The globe of the earth is directed and moves by magnetism, and at the same time it also coheres and is welded together in its inmost parts, so that it becomes solid."

To these remarkable indications must be added, however, others as remarkable in respect to the magnet. There is something startling in certain of the chapters relating to the electrical properties of the earth and



the attraction of the planet for substances upon its surface. He gives a drawing (fig. 1), of which I present a copy, in which he depicts a magnetic globe or orb, and indicates how magnetic poles or bars arrange themselves in respect to the axis of the globe.

One of the most interesting chapters on this matter of terrestrial magnetism is chapter xvii. in Book I., "*Quod globus terræ sit magneticus.*" "The globe of the earth magnetic; the magnet; and how the magnetic stone has all the primary forces of the earth; and that the earth through the same forces maintains its fixed position in the universe."

This chapter begins:—

"Before the cause of magnetic motions, the demonstration of facts concealed during so many ages, and experiments (those true foundations of human philosophy) are brought to light by us, that new and unheard-of opinion of ours must be stated and laid before the eyes of the learned; which, when it has been debated and supported by certain probable reasons, it will indeed be established as firmly as anything which hath ever been seen or proved in philosophy by ingenious arguments or mathematical demonstrations."

Gilbert's opinion, thus prefaced, is, that the earth with its masses of rock and water, and all its parts, whether seen or hidden from human sight, is a magnet, and is possessed of all the qualities known as magnetic. "I consider," says he, "the earth to be a solid substance, firmly cohering in its primary form, and (as in the other globes of the universe) endowed with a well-knit form, which maintains its position and fixed polarity, and revolves with a determinate motion from an implanted power of movement, even as the magnet, beyond all bodies seen by us, possesses a true and

genuine character, little injured and deformed by external evils, which is a homogeneous and true part of, and which has been separated from, the body and volume of the earth.

“Such is our earth in its interior parts, a body possessing a homogeneous magnetic nature; and on such a perfect foundation stands the whole nature of terrestrial things, which under our diligent scrutiny everywhere shows itself, such as in all the magnetic metals of the earth, in veins of iron, in all kinds of clay, and in most varieties of earth and stone: whereas the pure element of Aristotle, and the vain terrestrial phantasm of the peripatetic philosophers (that simple substratum of all things) is rude, inert, cold, dry, dead, of no vigour, hath never appeared to any one except in dreams, and is of no effect in the nature of things. To speak plainly, all the solid earth, wherever it is seen; even that earthy matter which has not been purified from coarse and humid elements, such as clay, mire, and material gathered from putrid substances; whether they have been injured by the imperfection of various mixtures, or fallen to pieces through their grossness like marl; all these, whether they have been prepared by fire alone, or are freed from their superfluous moisture, are attracted by the magnet, and as by the magnet so also are other bodies magnetically attracted and set in order by the earth itself; and through their implanted force do they arrange themselves according to the method and order of the world. So also every fragment of the earth, by true experiment, shows all the force of the magnetic nature, and obeys the globe and universal principle of our earth throughout its various motions.”

In another part of his work he gives the following

diagram to show how a needle, floated by means of a cork in a goblet of water, exhibits both variation and declination if it be rendered magnetic (fig. 2).

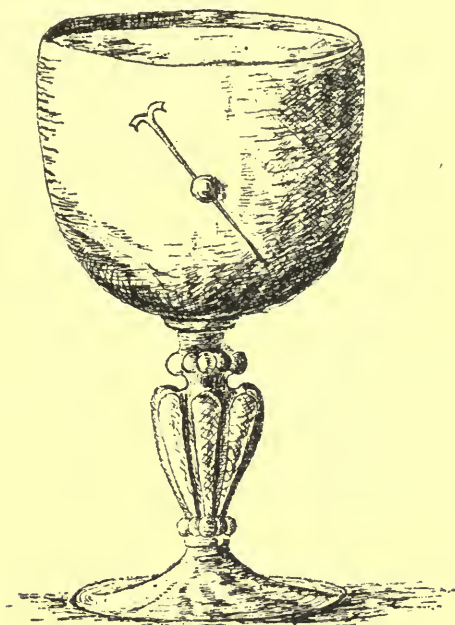


Fig. 2.

In chapter xii., Book I., he compares magnetic force to vital force. Magnetic force is life, or resembles life, and surpasses human life in many respects, while it is bound in the organic body.

“The magnet is wonderful in innumerable experiments, and is, as it were, a living force. This is the one remarkable virtue of those which the ancients considered to be life in the heavens, in globes and stars, in the sun and moon. For they considered that such varied motions could not be maintained without a divine and living force; or that without it, vast bodies could revolve in fixed terms, or that wonderful powers

could be infused into other bodies through this primary form of the globes themselves. The ancients, the whole Platonic school, the Egyptians and Chaldaëans, affirm that the universe is endowed with life. On the other hand Aristotle thinks that the elements are inanimate, the stars animate. I myself think that the whole creation, all globes, all stars, and the glorious earth itself, are governed from the beginning by a proper and determinate life, and have their movements of self-preservation. Although there are not in the stars, the sun, or the planets any organs which can be recognised by us, yet they live. If there be anything of which man can boast, assuredly it is life ; and God Himself (by whose will all things are ruled) is intelligence, is mind. Who is he, then, that will demand organs in the divine mind, which overpasses all the framework of organs, and is not restrained by material organs ? But in the different bodies of the stars, the implanted force acts otherwise than in those divine things which are supernaturally ordained ; and in the stars the force acts otherwise than in animals ; in animals otherwise than in plants. Certes, it were a miserable condition of the stars, a base lot of the earth, if that glorious dignity of life were denied them, which hath been granted to the worm, the ant, the moth, the plant and the toadstool."

#### LAST DAYS AND WORKS.

Morant is careful to tell us that Gilbert left behind him another work "*De Mundo Sublunari Philosophia Nova*," which work, still in manuscript, was retained in the library of Sir William Boswell, Knight. Nearly fifty years after the death of its author it was published from Amsterdam, by his brother William Gilbert, junior, a

proctor in the Court of Arches, with a dedication to the unhappy Prince Henry of Wales, friend of Walter Raleigh, and son of James the First.

According to the same biographer, Gilbert invented two ingenious instruments for seamen to find out the latitude without the help of sun, moon, or stars. The instruments were made public by Thomas Blondeville, in a quarto work published in London in 1602.

The annual pension left by Queen Elizabeth to our philosopher physician was not long enjoyed by him. The queen died on March 24th; the philosopher on November 30th, 1603. He was never married.

In many respects Gilbert resembled Harvey, particularly in having been much beloved and honoured by his brothers, of whom he had four, Ambrose, William, Jerome, and George. Over his tomb in the Church of the Holy Trinity, Colchester, the brothers Ambrose and William placed a Latin inscription, which my learned brother Munk has copied into his Roll of the Royal College of Physicians, and which I supplement in English :—

AMBROSE AND WILLIAM GILBERT HAVE PLACED THIS TOMB,  
IN MEMORY OF BROTHERLY PIETY  
TO WILLIAM GILBERT, SENIOR, GENTLEMAN, AND DOCTOR OF MEDICINE.  
THIS THE ELDEST SON OF JEROME GILBERT, GENTLEMAN,  
WAS BORN IN THE TOWN OF COLCHESTER,  
STUDIED THE ART OF MEDICINE AT CAMBRIDGE,  
PRACTISED THE SAME FOR MORE THAN THIRTY YEARS AT LONDON,  
WITH SINGULAR CREDIT AND SUCCESS,  
HENCE, CALLED TO COURT, HE WAS RECEIVED WITH HIGHEST  
FAVOUR BY QUEEN ELIZABETH,  
TO WHOM, AND TO HER SUCCESSOR JAMES, HE SERVED AS CHIEF PHYSICIAN.  
HE COMPOSED A BOOK CELEBRATED AMONGST FOREIGNERS  
CONCERNING THE MAGNET, FOR NAUTICAL SCIENCE.  
HE DIED IN THE YEAR OF HUMAN REDEMPTION, 1603, THE  
LAST DAY OF NOVEMBER,  
IN THE 63<sup>RD</sup> YEAR OF HIS AGE.

The monument bearing the above epitaph remains in



fair preservation in the old church of the Holy Trinity ; and hard by still stands Gilbert's house, once the Tymperley's. Friend Henry Laver, of Colchester, Surgeon there, and excellent antiquarian scholar, to whom I am much indebted, has taken a photographic view of the residence as it now is, which view Bertram Richardson has transferred to paper, as a fitting conclusion to this short history of the First Electrician.



GILBERT'S HOUSE AT COLCHESTER.

## SCIENTIFIC BASES OF THE TEMPERANCE REFORMATION.\*

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**A**LL through the temperance ranks the arguments which appeal most strongly to the sentimental part of men's nature are being enunciated with vivid force. Nay! this enunciation is not confined to the abstinence world. Hosts of men and women of the temperate sort, who are quite as earnest, in their way, for national reformation, quite as energetic in denouncing the evils of intemperance, and quite as strong in declaring the privileges and beauties of temperance as we are, go with us, as freely as we go, up to the act of quenching the appetite for alcohol to which we, logically, proceed. I do not, therefore, consider it my duty to dwell on those parts of our argument which touch on the sentimental side. Let the good and the sympathising continue this good and sympathetic and truly evangelic work. What matters it to science if in the United Kingdom fifty thousand persons die each year from alcohol? What matters it if the Judge be true who said that 90 per cent. of all

\* From the Presidential Address delivered in Section I. at the National Temperance Congress, Liverpool, Tuesday, June 17th, 1884.

the criminal cases were from the same cause? What matters it if workhouse and asylum are charged in regular course from the same cause? What matters it if every act of sedition and rebellion, if every pound of damnable dynamite, be made and laid under the influence of the agent that brings it into activity? What matter to science? Nothing. The greatest good for the greatest number is her prescription, and if this thing alcohol, bad as it may be, is necessary, let it rage and live like storms and hurricanes and earthquakes, which smite and kill, and yet out of the very womb of death bring forth the most perfect forms of life.

I shall do my part best by letting sentiment remain in other hands. For the moment I will take my stand with those who cry for the greatest good for the greatest number, be the consequences what they may. If all the evils incident to the consumption of alcohol be as bad as they are made to be; if England be doomed to wreck and ruin, to mental and physical death from it; still, if it be a part of the great natural design, if it be "absolutely necessary to man," as one of our leading publicists has recently declared it to be; if it be as necessary as fire, let it remain. Make every protection against it as against fire; insure against it; have stations to which to carry those who are injured by it; do everything that can be done to remedy all the misery it accomplishes; but, if it be "absolutely necessary," let it remain.

#### IS IT NECESSARY?

The scientific basis of the Temperance reformation will be laid if it can be proved,—

1st. That this thing alcohol is not necessary ;

2nd. That as a thing unnecessary it differs from many such things in being an evil thing.

I will try to prove these positions.

I need not try to prove more.

In this effort I will be careful to put forward no statement that shall overstep the views of the thoughtful men of science, whether abstainers or not, who have recently examined the questions relating to the action of alcohol upon man and life. If a point occurs on which there is reasonable doubt I will not press it, and I will give up all special personal views which I may have urged from direct experiment and observation, unless they have fallen in with general acceptance or consideration.

#### AGAINST THE ASSUMPTION OF ALCOHOL AS A CON- STRUCTIVE FOOD.

After a long struggle of opinion there is now all but uniformity that alcohol supplies no structure to any part of the body.

There was never at any time the slightest scientific proof that it did; but it was assumed so to do. In my student days I was zealously taught that wines and spirits and ales made blood, that blood was liquid flesh, and that they therefore made flesh or tissue. In course of time, as the discoveries of Majendie disclosed what varieties of food really sustained the animal tissues; as the discoveries of Schwann and Schleiden disclosed the cellular formation of the tissues; and, as the discoveries of Liebig unfolded the chemical relationship of various foods, it has come out, by clearest proof, that alcohol does not belong to the class of organic food substances that can build up the nitrogenous or vital parts of an animal body.

That it was not necessary for this purpose has been proved and is proved millions of times over, in the existence of the millions of millions of living things that exist without resort to the use of it. If the horse and the cow generally—to use an illustration by Dr. Munro—required alcohol for their vital sustenance, the human world employed in making this substance would be sufficiently taxed. But if all animals demanded it for the same natural reason, the whole human family, doing nothing else, would barely find time to replenish the whales, the elephants, and the hippopotami alone with the materials for their bodies, to say nothing of the requirements for the smaller kinds of animals, birds and beasts, reptiles, and other things of life. Common sense, which is the best form of common science, would point to this evidence of uselessness as all-sufficient to show that alcohol contributes, by necessity, to no structure. But, in further support, special science comes to our aid, and shows us beyond doubt that for the formation of the vital structures one particular elementary base is required, namely nitrogen, with sulphur, phosphorus, carbon, hydrogen, and oxygen as other necessary elements, combined in a distinctive combination. She further shows us that these structure-building nitrogenous compounds are amply provided for us, ready made, in the vegetable world.

I press this argument no further. There is not an authority in the world of science who would now presume to advance the old notion that alcohol feeds the living structures, or is even indirectly necessary for their existence, or that nutrition, growth, construction are supplied by alcohol. Everyone who has to treat those unhappy members of society who come nearest to the trial of living upon alcohol knows that these are



the most degenerate physical specimens extant, and the best evidences of the truth of my thesis.

There are some, perhaps, who would hold still to the exploded view, by assuming that one structure dead in its form, and dangerous as death in life, if it preponderate and replace living tissue, I mean fat, may be constructed out of alcohol in the body. There is, however, no evidence even in favour of this view. We see in our cattle-shows swine fattened until they cannot open their eyes, and cattle fattened until they can scarcely breathe, and all without a drop of alcoholic spirit as the fattener. On the other hand we learn, from experiment, that animals freely supplied with alcohol, minus all saccharine or starchy substances in connection with the supply, lose deposited fat as well as muscle. Apart, therefore, from another question which some have raised, viz., whether alcohol would be rendering a good service to man if it did promote fatty deposit in his living tissues, the balance of evidence is in favour of the view that it does no such thing, and I conclude this part by the declaration on the basis of scientific fact, that alcohol contributes nothing that is necessary for building up the animal organism of man or lower animal.

#### AGAINST ALCOHOL AS A SOURCE OF VITAL POWER IN THE BODY.

Under the term *vital* power, we may cover all the physical nature of the animal that is other than material. We may include the *calor vitalis*, the living fire as indicated in the living man, by the heat which radiates from him; the motion that shows him to be alive; the mental acts which excite the motion; the concentrated activity; the static force ready to move in obedience to something more distinct and

refined still, which our most subtle research does not yet follow, the egoic entity, the "I will."

This vital power, the associated imponderable connected with the constructive organised material and animating it, requiring, like the constructive matter, food for its manifestation, without which the body is dead—does this depend, of necessity, on alcohol for its supplies? Is an animal body made warm, strong, active, precise in activity, felicitous, by alcohol?

The answer to this question, in application to the small section of life called the human family of life, and part of it only, was an affirmative answer until quite late time. The doctrine of the schools has been, alcohol for warmth, alcohol for strength, alcohol for activity, alcohol for felicity. The poets have re-echoed the schools:—

"Wine's the soul of man below."

Perchance! But where is below?

The common sense of mankind would infer, if it were thoughtful, that all provision was made naturally for the animal powers without alcohol. This fact is as patent as that relating to construction, when it is surveyed on the large scale. Animal warmth is provided for the polar bear, the seal, the northern whale, without any reference to alcohol. The Esquimaux keeps warm without it, and could not easily get it if he felt he required it. Every child that is born is in want of animal warmth more in its first days than at any period of its life; but it does not depend on alcohol, naturally, for its supply.

Animal strength is provided for the elephant, rhinoceros, camel, lion, eagle, and other strongest animals without any reference to alcohol. The strongest

and finest built men in Europe, as Sir William Fairbairn tells us, are the boatmen or rowers to the caiques in Constantinople, men who, during their most arduous work, keep up their strength without resort to alcohol; and everyone who is justly observant will find similar illustrations in his own and other communities.

Activity and precision in activity are notoriously to be obtained without the aid of alcohol. All well-trained lower animals, from finest race-horses or war-steeds down to dancing dogs, are proofs of precision in activity without resort to alcohol. Man follows in the same wake; if he wishes to shoot well, row well, walk well, fight well, write well, think well, he abstains from alcohol.

And felicity, too, the last-named of the imponderable powers, is, with equal obviousness, best obtained without any reference to alcohol. The first period of life, when life is fairly surrounded by earthly comforts, is the happiest, and that is the abstaining period. The life that is least embarrassed with sadness, least troubled with emotion of sorrow, most equable in its felicity, is that life which Dr. James Johnson dignified as the life gliding most tranquilly on its course, neither exhilarated nor depressed by alcoholic aberration.

These are common matters of every-day science. They are backed by the science which is at once experimental and severe. The most accurate experimental observations demonstrate that the physical action of alcohol is to reduce the animal warmth; to bring down the effective power for physical and mental work; to render sensorial or muscular precision defective; to produce excitability of nervous action; and to keep the mind in continual change of cast and character, over-resolute, irresolute, uncertain, moving from feeble-

ness to fury, from the brief madness of anger to the persistent madness of confirmed insanity.

How on a large scale alcohol lessens warmth of body, power of limb, precision of movement, decision of character, every shivering, trembling, uncertain, undecided victim of it whom we meet in the streets shows; every person who passes from ebriety to drunkenness shows. But Science, in her severer readings, detects that the failure of strength and precision begins much sooner and under much lighter provocation than is supposed, by the majority of mankind, to be possible. All persons admit when they see a human being passing, step by step in regular order, from his natural self into excitement, from excitement into lip palsy, from lip palsy into inarticulate palsy, and from that into limb palsy, until he collides with all that stands in the way between himself and the earth, that he has lost power, and that the gravity of the earth has brought to shame the levity of his conduct. But few conceive that the feebleness and unsteadiness were all through mere matters of degree, and that both feebleness and unsteadiness commenced so soon as the alcohol began to produce its primary effects.

My own experiments many years since gave proof of this fact, and recently, by a more refined series of experiments than mine, Dr. Ridge has shown that so small a quantity of alcohol as four fluid drachms, or half a fluid ounce, is sufficient to disturb the eye for correct vision, the muscles of the hand for detection of weight, and the sensory tip of the finger for the sense of touch. Following up this most suggestive line of research I have detected, by observations with the electric balance, that the refined sense of hearing is, in precisely the same manner, interfered with by the

action of alcohol in the most primary stage of its action, and by doses considered usually of no moment.

The measurement, scientifically, of the felicity derived from alcohol, shows, like all else, short commons. We men of science who follow, practically, the medical part, who, in the professional confessional, learn more of the unhappiness of human nature than all the world beside, hear of nothing so frequently, so solemnly, as the anguish produced in the happiest natures, as they were by nature, when touched by this subtle destroyer of happiness; a destroyer which makes even lower animals wild, drooping, sad, when they are taught to take it, and which ought to claim for its physiological definition, in regard to its action, dispenser of chronic sorrow.

On the scientific basis of the temperance reformation we may, without hesitation, take our stand as total abstainers, and declare ourselves, without offence, without quarrel with anyone, logical in our deductions from scientific fact, whenever the question of natural necessity comes into dispute. We know in what we believe. We know we are at one with Nature in her own designings, and that in controversy with the alcoholic constitution we are not in controversy with man naturally constituted, but with man constituted on an artificial system which endows him with appetites, sentiments, reasons, that would be unknown to him if he abstained so effectively as to be free of an artificially-induced aberration.

#### THE RESULTS OF THE UNNECESSARY.

I turn now from the question of necessity to that of resultant evils arising from the argument of necessity and the practice founded upon it.



It has always appeared to my mind, since I began to study the question upon which I am entering, that if there were necessity for alcohol, and if I, from the scientific side, should feel forced to back up the necessity, I should be driven nevertheless to admire those who would make any sacrifice rather than endure so great an evil. The mere physical injuries to our race are so overwhelming they themselves should crush out all affection for the cause exciting them, and should lead to the discovery of something less deadly for its replacement.

When, however, it is clear that the necessity does not exist, then the mere mention of the evils is sufficient to crown the condemnation. Happily, on this subject the concurrence of scientific opinion and of expression, founded on unmistakable facts, is in no sense uncertain. There may be minor differences of opinion, but the grand results are one, and on the basis named the temperance reformation is firmly laid.

WHAT ARE THE EVILS WHICH STAND OUT BEYOND  
QUESTION ?

The first evil is a definite calendar of disease largely peculiar to the human family, and incident to the consumption of alcohol.

Touching this calendar of disease, it is well to note the words I have just used, *peculiar to the human family*. In the lower forms of life, in our domestic animals, some of the states of disease incidental to man do not exist. Amongst these animals delirium tremens has no place; the peculiar disease of the liver, called commonly gin-drinker's liver, has no place; the various forms of Bright's disease of the kidney are exceptionally present. Yet it is certain, from my own experimental

observations, that all these diseases can be induced in these lower animals by training them to the use of alcohol, and that other diseases, including the *mania à potu*, may be added to the list of inducible maladies.

But the calendar of human diseases from alcohol—*morbi alcoholici*—is so large, it makes a distinctive study of itself, and now that the eyes of physicians are opened to recognise the truth, the study is expanding. When I, twenty years ago, gave the name of alcoholic phthisis to one particular class of consumption of the lungs induced by alcohol, the term was considered as an innovation, and was for a time resented. The late Dr. Francis Sibson, Senior Physician to St. Mary's Hospital in London, defended me in the use of the expression. Dr. Anstie followed, and in time the word "alcoholic," as an adjectival distinction for denoting origin of various affections, has come into wide acceptance. Thus we are now accustomed to hear not only of alcoholic phthisis, but of alcoholismus, alcoholic eclampsia or convulsion, alcoholic delirium, alcoholic epilepsy, alcoholic cirrhosis, alcoholic paralysis, alcoholic renal disease, alcoholic cardiac disease, alcoholic cerebral disease, alcoholic fatty degenerative change; and, latest, from the pens of one or two of our ablest physicians, of an alcoholic dolor or pain in the extremities, which has hitherto passed as an undefined affection.

I notice, also as a fact, very generally admitted amongst advanced medical scholars, that the courses of acute and chronic diseases, due primarily to other causes than alcohol, are much intensified, in respect to danger, amongst those who even slightly indulge in spirituous beverages. This great clinical truth is not new in medicine. It was declared with impressive

emphasis by one of the shrewdest practising surgeons who ever lived, Sir Astley Paston Cooper. It was re-affirmed by another surgeon scarcely less observant and able, the late Mr. Higginbottom, of Nottingham. I have dared in these days to follow these distinguished men in their teaching, but I have said nothing that could be more firmly said than by two who have followed me, Professor Flint, of New York, and Professor Alonzo Palmer, of the University of Michigan.

Professor Flint observes:—"The toxical condition called alcoholism enters directly into the constitution of many affections, such as cirrhosis of the liver, fatty liver, epilepsy, muscular tremors, gastritis, pyrosis, and various dyspeptic disorders. Indirectly, alcoholism favours the production of nearly all diseases by lessening the power of resisting their causes, while it contributes to their fatality by impairing the ability to tolerate or overcome them."

Professor Palmer, in his recent work on the "Practice of Medicine," the most truly practical volumes that have appeared on the subject since Watson's famous lectures, remarks upon alcohol:—

"Absorbed it has a special attraction for the nervous tissues, for the brain, and also, though to a less extent, for the liver. It produces more or less paralysis of the vaso-motor nerves; it changes the relation of the blood and tissues, it diminishes oxidation, it causes congestion of the liver, the kidneys, the lungs, and the brain. It generally increases the sugar in the liver, and the water secreted in the kidneys. It increases the tendency to the production of abnormal fat in the tissues generally, and tends to produce fatty degeneration. Above all, it acts chemically upon the soft nervous tissue.

"It causes, in its continued use, the proper nerve

elements to waste, a serous fluid to be effused ; it induces in the nervous matter the production of granular fat, and it causes development of the connective fibrous tissue. All parts of the body under its poisonous influence have a lower vitality."

Such is one of the latest deductions of science on the subject of the action of alcohol on the living organisation. I am prepared to hear it said that the references made above do not refer to the moderate use of alcohol. Professor Palmer wisely forestalls this argument by adding that no one has yet been able to define the limits of moderation. That is true. What would be moderate use in one, would produce some of these changes in another, and in all there would be injury.

The evidence of science is decisive as to certain local effects of alcohol on the organism, and especially on the liver, the brain, and the heart. To what extent the heart suffers, even from so-called moderate doses of alcohol, has been taught of late years with what amounts nearly to mathematical accuracy. By the sphygmograph the alcoholic heart may be made to pulse out and write out its own history. Four ounces of alcohol per day make the heart of an adult man beat 12,960 times oftener than it would otherwise do, which means the labour of raising nearly fifteen tons' weight one foot. During this labour the heart also beats more strongly so long as it is directly under the influence of the alcohol. It must do so because its own circulation is quickened. Its own vessels flushed with blood, and the tension of resistance of vessels at the extreme parts reduced, it works with a rapidity and force, derived from its own excessive blood supply, which is in excess to the degree stated, and which is wearing in proportion. To what extent the wearing takes place is best com-

puted when the mechanism of the organ is considered. With each stroke eleven segments of fine membranous valves are brought into sharp tension, a tension sufficient to yield an audible sound. Whether each of these finely adjusted and delicate valves—six of them not larger than a small finger-nail each, and the largest half an inch square—shall or shall not be made tense 12,960 times per day beyond what is required is a question a child might rightly answer. As a matter of fact, nature answers it surely enough in the distended, bagged, impaired condition in which the valves are found, frequently, amongst those who for a few years subject themselves to alcohol in doses generally considered to be free of all danger, and designated moderate.

I need not follow this section of my argument further. The production of a certain extended calendar of disease by alcohol in the human family is, so long as that family takes alcoholic drinks, inevitable. On that basic fact alone we have a sufficient scientific reason for the reformation we demand. If bread by its use produced the same unvarying phenomena of disease, we should be scientifically justified in demanding that even bread itself should be discontinued and replaced by some less deleterious food.

One reason leads to another. A certain measure of matured disease affecting vital organs, whatever the cause at work may be, is sure to lead to a certain reduction in the value of life, and to a certain excess of mortality.

That such a result is shown through alcoholic disease is so positive that the knowledge of it is being acted upon with the severity of commercial balance and exactitude. There is no occasion now to go to books of refined scientific character to grasp this truth; unpoeti-



cal actuarial day-books and ledgers, directors' reports, and shareholders' votes record it.

It was not my intention to refer to any of these commercial results when I sat down to compose this Address, because I did not suppose I could say anything new. But while the Address was in hand I have received a copy of the *Insurance Guardian*, an able paper, which is in no sense specially wedded to our cause, though it is open to honest conviction; and there I find an article bearing on the returns, just published, of the "Sceptre Life Association." This Company has two sections, a General and a Temperance section. It has 5,730 in the general, 3,270 in the temperance. In its last annual computation it turned out that the deaths in the general section were 68, or rather less than 12 per 1,000 living at risk; and in the temperance section 12, or less than 4 per 1,000 living at risk. But lest it might be supposed that this was an exceptional return, as indeed it was, the deaths from the beginning, twenty years ago, have been compared, and have shown 12 per 1,000 at risk in the general, 7·8 per 1,000 at risk in the temperance division. In the past five years the expected claims in the temperance section were 153. The claims actually demanded were 53, or about one-third of the number expected.

No more scientific argument could be found in defence of abstinence as compared with moderate drinking. If a late advocate for four ounces of alcohol a day could manipulate, after his fashion, the 3,270 members at risk on the temperance side of the Sceptre Life Association, it would be rather an unpleasant position in a year or two to be the actuary or board of that now excellent institution. I feel morally sure that in its general section the members are not subjected to so

dangerous a toxic practice as the consumption of four ounces of alcohol daily.

That part of the scientific basis of the temperance reformation, which is made up of facts relating to the better value of life and activity of the abstaining community, is of all others the most obviously sound. Our opponents may trample on it as they like, sneer at it as they like, fire at it their heaviest missiles. They will not move it.

#### DISEASE AS LIGHTENED BY ABSTINENCE.

I could follow up the history just rendered by a reference to other histories bearing on sanity and insanity, suicide and heredity. Suffice it to say that the lesson would be the same.

There is one more topic on which, for the sake of faithfulness of testimony, I must dwell before I lay down the pen. I have been now thirty-four years in active medical practice. Except during brief vacations, I have never been out of harness, and I do not think I have been more profoundly asleep to what is going on in the special world in which I live than the rest of my contemporaries. And this is the conclusion I have been led to, until it is an unshaken conclusion, that amongst those who practise total abstinence, not only is mortality much reduced, but, when recoverable sickness falls to them, the severity of it and the period of it are also remarkably reduced. In the acute stages of disease, the acuteness is, as a rule, less severe; in the convalescent stages the recovery is more rapid. These rules hold good in respect to the epidemic diseases, in respect to wounds and injuries, and in respect even to some diseases which are more immediately of hereditary type, as insanity, gout, rheumatism. I am further perfectly sure

that these results, although up to the present time drawn from a limited and fortunate part of the community, are affecting in the most signal way the material interests of the professors of medical art, to the extent even of lessening their numerical strength as an organised portion of the working community.

In the ten years ending 1881, the numbers constituting the body of religious teachers increased 14·7, which was as nearly as possible the increase of the population. The practising members of the legal profession, barristers and solicitors, increased 12 per cent. The professors of education, schoolmasters and schoolmistresses, increased 34 per cent. Musicians 38 per cent.; actors and actresses 30; dentists 45. But of medical men actually practising medicine or surgery, or both, the increase was less than 3 per cent.

Dr. William Farr, who occasionally enjoyed a fling at us abstainers, once told me his belief, in real earnest, that if all men became abstainers, the number of medical men required for public service would remain stationary, while the population continued in its regular progress. How fast that prediction is being fulfilled, these latest census returns better indicate than any further words of mine. They are most significant, and although we temperance folk must not take the credit all to ourselves, seeing that other great outlets of sanitary progress are adding their assistance to health, we may accept fairly that, in the opinion of the most eminent English statistician of the century, our health and life-saving labours enter largely into the calculation.

Finally, such facts run well with the figures which Dr. Ridge and the late Rev. Eardley Stanton, and other investigators, have collected in relation to the fewer days of sickness of the abstainers in provident clubs, as

compared with the days of sickness of those who do not abstain ; while they indicate, even from the present small scale on which they are represented, what an enormous change in the sickness of the nation would occur if all the members of these grand societies were of the same mind in practical support of our cause.

There is one order alone, the unparalleled order of Odd Fellows, which, accepting temperance lines as its lines, could in a single year produce an appreciable reduction in the national cost for disease and in the national mortality.

I implore the more thoughtful of our people who have not yet looked at temperance in the light here presented, to tarry a moment to look at this one of its bases which I venture to declare is laid. I pray them to examine the basis for themselves, to demolish it, if it be unsound, ruthlessly, completely. I pray them, equally, if it withstand their molestation, as I believe it will, to join in erecting upon it a part of that edifice of Temperance which, resting on a hundred other and equally sound foundations, will one day be the glory of an emancipated, chastened, purified, and beautified human world.

## OPUSCULA PRACTICA :

USEFUL NOTES FOR BUSY PRACTITIONERS.

“There are mites in science as well as in charity.”

BENJAMIN RUSH.



### TREATMENT OF NASAL POLYPUS BY SODIUM ETHYLATE.

HE affinity which sodium ethylate has for water led me, several years ago, to use it in cases of polypus of the nose, and I have so often found it an immediate and effectual remedy I think it is worthy of being brought in this light before the profession generally. The mode of procedure is extremely simple. The operator uses a long and slightly curved pair of steel forceps, the ends of which come to a fine point. He takes a piece of soft cotton wool, places it between the points of the forceps, and, twisting it firmly round the points, makes it into a good-sized, soft, flexible probe, like the end of a bougie. He saturates the cotton probe with the sodium ethylate, and then, having caused the patient to bring the polypus into view by sharp blowing of the nostril, he plunges the cotton, charged with the ethylate, into the polypus, and retains it in position for two or three minutes. On removing the cotton it com-



monly happens that the patient can expel the whole mass of destroyed polypus, in a semi-fluid form, by again blowing the nose sharply.

After the substance of the polypus has been destroyed, I, usually, make a second cotton probe, treat it with ethylate, as before, and pass it along the nasal cavity to the point at which the polypus would seem to have been attached. By this means the basis of the growth is destroyed as well as the growth itself.

Up to the present time the success of the mode of treating polypus, here named, has been, in my hands, most complete. I have had no instance of return of the polypus, and in the case of a young gentleman, brought to me by Dr. Stevenson, of Edgware Road, in whom there had been return after two previous removals by other methods, no return has followed the ethylate plan, although over two years have elapsed since I performed the operation.

The modus is sufficiently clear. The ethylate solution, in contact with the water of the polypus, is decomposed into caustic soda and alcohol. The alcohol coagulates the albuminous part of the mass, and the soda acts as a destructive caustic on the surrounding parts, so that the point of attachment is destroyed.

I regret to say that this mode of treatment, like that of the actual cautery, and the tearing process by the forceps, is painful. The pain is described as of a burning kind. Dr. Stevenson's patient was of opinion that it was much less severe than that arising from the other methods to which he had been subjected. It is not a prolonged pain, and it ceases almost immediately after the operation is completed.

There has been no instance of hæmorrhage after the above-named application of the ethylate, neither have

there been any severe signs of inflammation nor of foetid discharge.

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*CONDENSATION OF WATER ON THE BRONCHIAL SURFACE  
DURING NARCOTISM AND IN DISEASE.*

**I**T has occurred to me often to observe that the action of various narcotic vapours is greatly modified, during their inhalation, by the condition of the atmospheric air. When the atmosphere is extremely dry, the action of a narcotic vapour is greatly increased, and recovery from its effects is comparatively easy. On the contrary, when the air is saturated with water vapour the action is impeded; and if the air be at the same time cold and moist, the process of narcotism is often greatly impeded, while recovery is prolonged in proportion. But the fact I wish particularly to bring forward at this moment is, that when the body of an animal becomes profoundly narcotized and the insensibility is long maintained, during conditions in which the air is cold and moist, there occurs not unfrequently an actual condensation of water in the minute bronchial passages, which condensation leads to asphyxia, and, if it be continued, to actual death. This accident is best seen in cases of narcotic poisoning from hydrate of chloral; it may also be observed after poisoning from opium and other narcotics. I once saw it in the most marked degree in a case of acute poisoning from tobacco.

There are two causes which may produce this condensation. One is the obstacle to evaporation of watery matter from the surface of the animal membrane into the air. The other is the deficiency of force, in an animal whose general temperature is reduced, to raise water

into vapour and expel it from the pulmonary organs in the state of vapour.

Whenever condensation of water, from the causes named, is set up, the danger continues in an increasing ratio ; for as the condensation tends to cut off the air from contact with the blood, the temperature of the body (dependent always on the perfection of the respiratory process) decreases, until, at last, the respiratory change is prohibited altogether.

It is important in the extremest degree to remember the fact thus named in the treatment of cases of poisoning during which the animal heat is reduced. It will often turn the scale, in such instances, in favour of return to life, simply to place the body in a warm and dry air.

The fact is also of great practical interest in relation to some exhaustive diseases. The mucous rales and so called "death rattles" which occur at the close of many exhausting diseases, as well as the cold sweats that are seen on the surface of the body in syncope, in the later stages of phthisis pulmonalis, and on the approach of death in other diseases, are due to the cause I have named above—condensation. They are evidences that the body has not sufficient power or force to produce a rapid natural evaporation of water from the exhaling surfaces.

Lastly the fact is of moment, it may be said of great moment, in respect to the practice of diffusing water vapour in the bedrooms of persons who are suffering from bronchial disease. To what object the saturation of the air of the room is in such cases directed, has always been a mystery to me, and of its bad effect I have had many times the most signal proofs. The air is rendered cold by the water; volatile emanations from

the utensils are distributed by the water ; and, worse than all, the condensation of fluid on the already loaded bronchial surfaces is intensified by the water. It is like a process of slow drowning, to immerse a patient, who cannot take sufficient air into his blood already, in an atmosphere laden with water-vapour.

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*A CLINICAL METEOROLOGICAL CABINET.*

**M**ESSRS. Horne and Thornthwaite constructed for me in 1870, from a design I then planned, a portable meteorological cabinet for clinical purposes, which has proved of considerable service. The cabinet looks like a mantel-piece clock, takes up little room, and is not at all an unsightly object in the consulting room. It stands nine inches high, is six inches wide, and three inches deep. The case is of mahogany and the face of glass.

The cabinet is fitted up with a good aneroid barometer in the centre, a dry and wet bulb and a chamber at the back for ozone papers, when they are being used.

The cabinet requires very little attention to keep it in good working order. It is well to see every day or two that the water for the moist bulb has not evaporated, and that the case is kept free of dust. The cabinet can be carried in the carriage and can be placed in the sick room, in cases where it is necessary to maintain not only an equal temperature, but also an equal humidity of the atmosphere.

This question of the humidity of the atmosphere of the sick room is of equal importance with that of temperature in many cases of disease, especially in cases of acute and semi-acute disease of the lungs ; and a great

field for observation is open to patient clinical workers, in the direction of determining the degrees of humidity, which are, in particular instances, most commendable. As a general fact, subject to some but not many exceptions, a temperature of 60° F., with a humidity of 73° and a barometric pressure of 29·869, is the most favourable for diseases of the respiratory system.

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*TEMPERATURE AS A DIAGNOSTIC SIGN OF CHRONIC ALCOHOLISM.*

**H**AVE used for some time past the thermometer as an aid to diagnosis in doubtful cases of chronic alcoholism. The aid is a good one.

If in any case there be, with other symptoms of alcoholic poisoning, and without symptoms of local organic disease, a persistent low reading of the thermometer, a reading say of 97° Fahr., the evidence is almost absolute that alcohol is doing its certain mischievous work, and that the effects are becoming chronic. In the earlier stages of alcoholic indulgence, the temperature of the body may at times be raised, and, indeed, it is usually raised shortly after the indulgence, when the cheek is flushed and the brain excited. At a stage later a brief febrile paroxysm is followed by perspirations of a profuse kind and by decrease of the animal heat, the cold stage being of long duration if alcohol be not re-administered. At later stages still there comes a period when the body, charged regularly with the destructive agent, is never or rarely ever up to the natural heat. If there be perspiration it is cold, like a passive exudation, or condensation of water upon the skin, or a mixture arising from condensation and exudation.



Whenever this stage of persistent coldness of surface is reached, there is danger of many subsequent evils. The lungs are readily congested should there be a fall in the external temperature with moisture of the air ; the skin is easily made subject to scaly eruptions; the mind is disorderly, sometimes unduly elated, more frequently unduly depressed. The memory is wanting when it is most wanted, and there is frequent threat of paralysis in the lips or in the limbs, with starts of muscles and irregular sleep.

Bearing directly upon the above subject is a suggestion by Dr. Marshall, of Mortlake, in respect to the differential diagnosis of alcoholic coma—or deep drunkenness,—and apoplexy. Dr. Marshall suggests, in cases where there is a doubt whether a comatose person is intoxicated or is in apoplexy, that the thermometer should be used. In alcoholic drunkenness the temperature of the body will be found one or more degrees below the natural standard; in apoplexy the temperature will be found one or more degrees above the natural standard. As a general rule this observation is most correct, and is an important practical suggestion.



*PEROXIDE OF HYDROGEN AND OZONIC ETHER IN  
PHTHISIS PULMONALIS.*

HE peroxide of hydrogen received from me extended research as a remedy for phthisis pulmonalis at the time when I first introduced it into medicine,—1858-59. In one hundred and twenty-one cases I put the medicine to the test systematically and determinately. Of these cases sixty-six were in the first well-marked stage, thirty-one in the second stage, and thirteen in the third

stage. In six cases the phthisical affection in the first stage was coupled with chronic bronchial disease; in three cases phthisis in the second stage was coupled with chronic bronchial disease; and in two cases the phthisis in all its stages was combined with mitral valvular disease. The cases were nearly equally divided between the sexes; they were carefully diagnosed, but were not selected specially. The general impression of results as they occurred to me from time to time ran as follows.

In thirty cases of primary stage, treated by the peroxide solution only, I was driven to the conclusion that the solution had no specific effect in preventing the progress of tubercular deposit.

At the same time there were certain indirect advantages resulting from the remedy of no mean order. Thus it led to improved digestion; it exerted a salutary check on the overaction of the skin; in several instances it arrested hæmoptysis; and it gave decided temporary relief to the breathing during paroxysms of difficult expectoration and congestive dyspnœa. From the administration of the peroxide solution simply, I passed to its use in combination with iron and with cod-liver oil. In this way it seemed to increase the activity of the iron, improved the digestive power, and during congestive stages tended, I believe, largely to relieve oppression.

The value of the peroxide in phthisis was most marked in the periods of the first and second stages; but in many instances, during the final stage, it gave relief, accompanied by a soothing action which I do not remember to have seen from any other drug,—the quiet of an opiate without the narcotism.

A little later in the course of research I discovered

the mode of making the fluid since known as ozonic ether. I observed that by mixing a thirty-volume solution of hydrogen peroxide with absolute ethylic ether, and a small quantity of alcohol, the peroxide could be retained in the ether, and a useful solution secured. Since then I have all but invariably prescribed the peroxide, in one to two drachm doses, instead of the original form in which it was introduced. With spermaceti ointment ozonic ether makes an admirable unction for the skin for persons recovering from scarlet fever.

#### A SEA ATMOSPHERE FOR THE SICK-ROOM.

*Apropos* to this subject of the use of peroxide of hydrogen, I ought not to omit the services it renders in the sick-room when it is distributed in the form of fine spray. This mode of application gave origin to a paper which I communicated to the British Medical Association at Chester, "On a sea spray for the sick-room" (*British Medical Journal*, August 19th, 1865). The solution to be used and diffused as spray consisted of solution of peroxide of hydrogen (10 volumes strength), containing 1 per cent. of ozonic ether, iodine to saturation, and 2.50 per cent. of sea salt. The solution placed in a steam or hand-spray diffuser can be distributed in the finest spray in the sick-room at the rate of two fluid ounces in a quarter of an hour. It communicates a pleasant sea odour, and is the best purifier of the air of the sick-room I have ever used. It is a powerful disinfectant as well as deodorizer, acting briskly on ozonized test solutions and papers. Mr. Carl R. Schomberg has recently invented a large spray-producer which will diffuse the artificial sea air through a hospital ward.

*EUTHANASIA FOR THE LOWER CREA-  
TION—AN ORIGINAL RESEARCH AND  
PRACTICAL RESULT.*

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N the present short essay I have the satisfaction of placing before my countrymen the latest outcome of a research which has been a part of my whole life's work since I entered the profession of medicine well nigh forty years ago.

When I was quite young the blessed re-discovery of the process for rendering the bodies of men and animals insensible to pain during surgical operations was the grand event. To me it was the most wonderful of events, and it so possessed me, that from the hour of observation of the first administration which I had the opportunity of witnessing until the present hour, I have always been at work endeavouring in various ways, by the study of anæsthetics generally, and by the study of local anæsthesia, to improve the process, and to render it safe and ready for all occasions and all constitutions. But something else also occurred to me in the early days.

We had then, as we have had since, to place the

inferior animals under the influence of various new anæsthetics in order to determine if such anæsthetics were safe and practical. We had, moreover, to allow some of these inferior animals to sleep into death in order to discover the mode of death, and avert the danger of that catastrophe in the higher animal, man.

In the performance of these researches it occurred to me to think of the utilization of the facts I observed for the process of making death itself entirely painless to the lower creation, and I put my mind and hand to the art of setting forth and perfecting that design.

From the year 1854 to 1871, I made from time to time, and recorded, various essays on this subject; and in 1871 I communicated to the Medical Society of London a paper condensing the nature of my work, and explaining the progress I had made up to that time.\*

THE RESEARCH IN RELATION TO ANIMALS INTENDED  
FOR FOOD.

In the essay of 1871, the details of research supplied had reference to the painless infliction of death on animals intended for consumption as food for man. In it I first explained what I had gathered on the subject of mental or moral pain of death in different animals. Secondly, I referred to the physical pain to which animals are subjected in the various methods of slaughtering. Thirdly, I related the various attempts I had made to render the slaughtering absolutely

\* This paper was read before the Medical Society of London at the opening of the 99th session, on Monday, October 16th, 1871, Dr. Andrew Clark, President, in the Chair. It was entitled, "Note of a Preliminary Research to discover a Practical Method of killing Animals intended for Human Consumption without the Infliction of Pain."



painless. In this attempt I indicated the necessities that had to be met:—

1. The method proposed must be inexpensive and very simple; it must be a method which every slaughterer can carry out; it must not tax the courage of the man who kills, nor expose him to any personal risk.

2. The method must be rapid and decisive.

3. The method must not interfere with the flow of blood from the animal; it must not itself kill the body; it must only kill the consciousness.

4. The method must be such that no unnatural odour or taste shall be communicated by it to the flesh of the slaughtered animal.

I then proceeded to point out that there are two methods specially applicable: (a) the electrical shock; —(b) the administration of a narcotic vapour.

#### *Death by the Electrical Shock.*

Respecting the method of killing by the electrical discharge, I reported on the experiments I had made in 1869 with the large induction coil of the Royal Polytechnic Institution, with which I put to full test the practical value of electricity for the painless killing of animals.

I used, in these inquiries, twelve large Leyden jars, the whole representing ninety-six square feet of surface. In some cases the discharge was made in the ordinary direct way; in other instances the jars were set out in cascade on the plan devised by Benjamin Franklin. The results, as many who saw them will remember, were most striking. It was proved that the shock "in cascade" was the most fatal, but by both methods small animals, rabbits, and birds, were killed so instantaneously

that they actually remained in the exact position they had assumed at the moment the shock was given, so that it required careful examination to prove that they were really dead. In these small animals the bodies were left, after the shock, in a state of complete rigidity; but in a short time the rigidity subsided, and the flesh ate tender. The common idea that after death from electrical shock rapid decomposition ensues was disproved, for in all cases the bodies of the animals remained for several days free from decomposition. In another series of experiments, larger animals, sheep, were subjected to the shock, and in every instance unconsciousness immediately followed the application of the shock, the current being passed from the heads of the animals through the body to the hind extremities. The method proved very difficult to carry out in practice, for two reasons. Firstly, it was found that if the shock were so decisive that death took place absolutely, the animal would not afterwards bleed, while, if the shock were not completely decisive, the animal, during the flow of blood, evinced certain signs of returning consciousness, a phenomenon as remarkable as it was unexpected. Secondly, it was found that the administration of the shock was dangerous to the operators unless they took such care as could not be expected from all the men who are employed in the duties of the slaughter-house.

#### *Death by Anæsthesia.*

The electrical method of producing a speedy death failing as a practice, there remained the plan of inducing insensibility to the pain of death by the application of narcotic vapours administered by inhalation.

I reported that by this method the action of various

narcotic vapours had to be studied and tested ; different modes of administration had to be studied and tested ; and the length of time necessary for efficient administration had also to be studied and tested.

It was, further, a necessary condition that the administration of the vapour should neither excite the animal so as to render it violent, nor produce vehement convulsive movement; for such occurrences would cause injury to the flesh, and interfere with the skilful dressing of the carcase. It was necessary that the insensibility should be very quickly produced, otherwise the process of setting of the muscles (rigor) was rendered imperfect. It was necessary that the vapour used should be energetic in action, but at the same time applicable in the smallest possible quantities. Lastly, it was necessary that the insensibility induced should stop so far short of death, that when the blood-vessels were divided they would bleed freely ; in other words, it was essential to kill all parts except the heart ; for such is the popular feeling against the sale of animal food that contains blood, it were vain to suggest a mode of death that did not include the process of letting of blood.

In the earlier days of research with narcotic vapours, these conditions could not well be fulfilled. We required to learn the action of a great number of vapours before it could be seen what was the special action of vapours in their simple form, and what of different vapours in combination. These facts it had been my duty to learn, and success, so far as it is attained, rested on this line of study.

The four agents with which the mind is most familiar—viz., nitrous oxide, carbonic dioxide, sulphuric ether, and chloroform—were all in their simple forms inapplic-

able. Nitrous oxide produced much convulsive action, darkened the colour of the arterial blood, and darkened the flesh; added to which, it was an expensive agent. Carbonic dioxide was too slow in its action, produced convulsion, and darkened the flesh. Sulphuric ether was too slow, darkened the flesh, and communicated its peculiar odour to the tissues. Chloroform mixed with air was too slow, caused convulsion, and though it did not lead to any modification of colour, gave to the flesh, unless the death were so quick that the heart were paralyzed instantly, a peculiar taste and odour.

Three other compounds were effectual for the purpose. One was a mixture of vapour of hydride of amyl and methylene bichloride diffused by coal-gas; the second a mixture of carbon disulphide and methylene bichloride diffused by coal-gas; and the third chloroform diffused by coal-gas.

I induced insensibility with these agents in various classes of smaller animals, and also in sheep, with the best results. The advantages of these vapours consisted in the extreme rapidity of their action. When administered in the way I described they destroyed all sensibility in the space of eighty to a hundred seconds. I timed the process of narcotism on six sheep, and found it averaged one minute and twelve seconds for each animal. The mode of administration consisted in the administration of the narcotic from a metal funnel-shaped inhaler. When the animal—a sheep, for example—was brought by the slaughterer ready to be killed, two fluid-drachms of the fluid to be used were simply poured into the inhaler upon a layer of flannel or domette, and the inhaler was gently but firmly applied over the nostrils, like a muzzle. As a rule, the animal inhaled precisely as if breathing the common air, making no struggle, but

passing at once into gentle and deep sleep, the period of actual insensibility being marked by drooping of the head, closing of the eyes, and deep breathing, with a little noise in breathing, such as commonly attends very heavy sleeping.

At this time the butcher killed in the usual way, by dividing the vessels of the neck. The operation was accomplished without a single indication of pain. At first there was some hesitation in the flow of blood, but after a moment or two the blood began to flow freely; to use the common term employed by the butcher, "the animal bled well." The sensibility being destroyed only, the death was purely by the loss of blood, but the phenomena of death incident to the ordinary mode of killing were modified.

To explain more fully, I should premise that in the slaughter of animals by loss of blood, there are two definite series of convulsions. We may call them the primary or syncopal, and the secondary or fatal convulsions of bleeding. The first paroxysm indicates the moment when the connection, naturally maintained through the blood, between the volitional nervous centres and the muscles under their command, is broken by the reduction of the column of blood, and when the animal, unconscious and incapable of any self-act, lies in what we call syncope—lies, living only by the continuance of the functions of the mere organic life. The second convulsive paroxysm indicates the further extreme and fatal period, when the remaining thread of function, sustained by the spinal cord and the organic centres, is, in turn, broken.

In all warm-blooded animals these convulsions occur in precise order of time, and depend on the actual quantity of blood lost. In the sheep, the primary or syncopal paroxysm occurs when from thirty to forty



ounces of blood have been lost, after which event the further flow of blood is much subdued. The slower loss of from eight to ten ounces more blood leads to the fatal convulsive paroxysm, when the heart ceases its play.

In death from hæmorrhage *during the insensible condition produced by a narcotic vapour*, the primary convulsion was either suspended altogether or was much reduced in force, but the secondary attack, though less severe than under the common mode of killing, was always exhibited. It occurred even when the head had been removed from the body. It is, consequently, an unconscious and painless phenomenon.

The three narcotic compounds noticed above acted equally well in producing the quick insensibility required for securing painless death, and they left no trace of odour or taste behind. In proof of this I placed before the Society specimens of fowls that had been thus painlessly killed, in which the flesh was as perfect as could be desired. Even the blood and uncooked flesh were free of odour. Of the three narcotics the first and last were, on the whole, most applicable, for although carbon disulphide was admirable in its action, its smell at the time of using it was objectionable.

The mode of the administration of the narcotic vapours with the metal inhaler was sufficiently simple; but in order to remove any difficulty in measuring the fluid in every case, I devised an apparatus that could be suspended on a wall like an old-fashioned oil lamp, and that acted, when prepared, simply on turning a tap.

The apparatus was constructed on the simple plan of a Wolff's bottle; it consisted of a hollow metal cylinder for holding the narcotic fluid; from which there came a long escape tube of india-rubber connected with the inhaler or mouthpiece: another tube, running down

within the cylinder and below the surface of the liquid, was connected with a supply of coal-gas, and was armed with a tap. Thus the gas, when turned on, bubbled through the narcotic fluid, and carried it over freely, in vapour, into the inhaler. A very gentle current sufficed to charge the inhaler effectually; the tap governed the supply as it was wanted, and the waste of fluid which occurred from pouring out frequently from a bottle was saved. By this plan—which could be taught in a single lesson—the cost of rendering all the domestic edible animals, except oxen, insensible of death, need not, I showed, exceed one penny per head.

For a large slaughter-house I proposed to construct a different apparatus, after a model placed before the Society. This apparatus consisted of an enclosed passage, or narrow chamber, with four swing doors meeting in the centre, the doors opening by a simple push, and closing by means of a spring when the force for opening them was removed. Into the centre compartment of this enclosed passage the narcotic vapour, when it was required, was to be driven in at the top by gas pressure, as already described.

To narcotize the animal, it was to be placed in a small truck, which, by means of an endless chain, could be easily moved from one end of the closed passage to the other end. As the truck entered the passage it pushed open the first flap doors which opened inwards; these closed as they were passed, the second doors opened, and when the truck reached the centre they also closed, leaving the animal in the narcotizing chamber to fall into insensibility. Passing through the exit doors of this Lethe, it would be delivered over to the slaughterer in a state ready for the infliction of painless death.

This same apparatus on a large scale might, I thought, possibly be rendered applicable for oxen, when the necessity for the use of the pole-axe would be removed. On a smaller scale it might, I showed, certainly be employed for the painless destruction of dogs, cats, and other domestic animals not intended for food. Again, it might be used for narcotizing animals prior to the performance of painful surgical operations.

LATER RESEARCHES WITH CARBONIC OXIDE GAS, AS THE  
NARCOTIZER, IN A LETHAL CHAMBER.

Some researches on anæsthesia conducted in 1853 led me to an exposition of the anæsthetic action of the fumes of the *Lycoperdon giganteum* or common puff ball.\*

These fumes were found to be most actively narcotic, and on analysis of them by two independent observers, the late Dr. John Snow and Mr. Thornton Herepath, it was found that the narcotic present was carbonic oxide, CO. On this being determined I commenced to follow up the study of carbonic oxide, and in course of time employed it as one of the cheapest and readiest of the lethal gases for the painless extinction of life in the lower creation, using it frequently for narcotizing sheep, birds, and dogs.

In 1878 I constructed, for the first time, a lethal chamber in which sheep were introduced in order to be rendered insensible by this gas before being subjected to the slaughterer. The chamber was capable of receiving two sheep at once, and the carbonic oxide was made by passing common air in a simply-constructed stove over

\* Essay on the anæsthetic properties of the *Lycoperdon giganteum*, or common puff ball, read before the Medical Society of London on May 20th, 1853. Reprinted from the *Association Medical Journal*, and published by T. Richards, 37, Great Queen Street, 1853.

charcoal. The gas diffused through the chamber was sufficiently effective in its action to render the animals insensible to pain in a period of from one minute and a half to two minutes. When entirely unconscious they were removed from the chamber, and finally killed, by the butcher, in the usual way.

These animals had no sense whatever of the violent death to which they were subjected. They felt no more of the slaughterer's knife than the patient under chloroform who is about to have a limb amputated feels the knife of the surgeon. When they had lost the quantity of blood that is required to produce the phenomenon, the usual death convulsion incident to loss of blood occurred, but it was painless and very short in its duration.

The flesh of the animals—eight in number—killed in this painless manner was entirely unchanged. The gas combines so harmlessly with the tissues, it communicates neither odour nor taste to them, and is, in short, entirely innocuous to the flesh as food.

In the painless slaughter of animals intended for food there need be no hesitation in the selection of the narcotic. Carbonic oxide is the right agent in every respect.

The painless death for animals to be used as food might be put into operation at once in a properly-constructed abattoir in the case of sheep, lambs, calves, fowls, rabbits and other game, pigs, and perhaps oxen. Of the oxen I am not sure, the pole-axe being so very speedy and effective when it is properly used.

For sheep the narcotic method is specially appropriate. Sheep come under the influence of the narcotic with singular facility, and are saved from what is to them a very painful death.

A LETHAL CHAMBER FOR ANIMALS WHICH HAVE TO BE  
DESTROYED FROM DISEASE, OLD AGE, INJURY, OR OTHER CAUSE.

I suggested in 1869 to the Royal Society for the Prevention of Cruelty to Animals the adoption of painless death for animals that have, of necessity, to be destroyed, and devised then the plan of a lethal chamber in which carbonic oxide alone or combined with some other rapidly-acting anæsthetic should be used as the narcotic. The proposition led to a considerable amount of discussion and correspondence, and caused many persons to write to me for an outline of the process. From this general discussion of the subject there has been some practical issue in America, and within the present year I have been enabled, I am glad to say, to carry out the project in this metropolis on a scale of the most thorough kind. Through the generosity of one benevolent man, Mr. Kennet, I have constructed at the Dogs' Home at Battersea a large lethal chamber in which from fifty to one hundred dogs can be narcotized at once, and can be allowed, without awakening, to pass from sleep into death.

On Thursday, May the 15th of this year, I put the chamber, for the first time, into practice, by passing into it thirty-eight dogs which had to be killed. The animals were quickly asleep, and when removed from the chamber were all lying precisely as if asleep, but every one dead.

On the 21st of May fifty-four dogs were submitted to the same mode of death in the lethal chamber. They fell asleep in the most direct and easy way; and on removal from the chamber were all found lying as if asleep, but quite dead.



The same process has now been repeated many times on batches of dogs varying from fifty-three to eighty-four at one time. On the whole five hundred dogs have been in this way made to sleep into death,—have been submitted, that is to say, to death with no more sense of pain than is felt by every human being who goes to sleep from the hand of the administrator of an anæsthetic vapour. The death is the easiest it is possible by any art to devise. First sleep, then death-sleep, then death.

The lethal chamber is an air-tight chamber built of wood, with double walls holding a layer of sawdust between them, so as to sustain an equable temperature, and secure an equable diffusion of the lethal air within the chamber at different seasons. The chamber is capable of holding two hundred cubic feet of lethal air, and is constructed to receive a cage having a capacity of one hundred and forty cubic feet.

The cage runs easily on wheels into and out of what may be called the central nave of the chamber. As it enters it pushes before it a valve screen, which prevents the escape of lethal air; it also pushes before it, at the further end, a movable screen to allow for the displacement of the air caused by its entrance.

The chamber is charged with the carbonic oxide from two condensers, one on each side, constructed for me by Mr. Clark, of the Hygienic Heating and Lighting Company, Park Street, Islington. The vapour enters the chambers in two small aisles right and left of the central nave, and on its way is made to take up from two separate small reservoirs a current of vapour of methylated chloroform containing bisulphide of carbon. These vapours, both powerful anæsthetics, diffusing through the lethal chamber, maintain the

lethal atmosphere within the chamber after it is closed and when it is not in use.

When the lethal chamber is required for use, the carbonic oxide is generated freely in the stove condenser, one pound of charcoal being used for the production, excluding loss, of every twenty-five cubic feet of gas. For three hours the gas is diffused into the chamber at a temperature of about 75° F., and at the rate of one hundred cubic feet per hour. The chamber is thus well filled with a lethal atmosphere, through which finally the methylated vapour is rapidly diffused.

The chamber ready, the animals are put into the cage outside of it. The first doors of the chamber are then thrown open, and the cage pushed before it the valved screen spoken of above is run in. Its own end closes up the entrance; but to make all perfectly airtight, the outer door of the chamber is immediately closed. The animals are now immersed in the lethal or narcotic atmosphere. The whole time of introduction of the cage and closing of the chamber is less than half a minute.

It is the common experience in narcotism, both in man and the lower animals, that there are four degrees or stages towards the final stage. The first is a short stage of excitement; the second of convulsive or starting action with some delirium; the third of insensible prostration; and the fourth of entire collapse, ending, if the narcotism be prolonged, in death. I have generally observed in men under chloroform, that if all goes well, the first stage lasts from one to three minutes; the second from two to three minutes; the third as long as the administrator chooses to sustain it by the judicious continuance of the vapour according to the requirement of the surgeon. In the

lethal chamber the percentage of lethal vapour is so great, there is practically only one stage before that of collapse. At first the animals are, as a rule, completely quiet; then they seem, one by one, rapidly to fall into deep sleep, often with heavy snoring; finally, with a series of short sharp barks, in some cases, they fall into death, the perfect placidity in which they are discovered after death indicating that they have passed imperceptibly from sleep into death.

It is important, however, for me to record that all animals after they have fallen into deep sleep under the lethal vapour do not pass into death with equal rapidity. About three per cent. of animals, after the narcotism is fully established in them, show such a peculiar tenacity of life that they may continue to breathe for some time after the rest of their comrades are dead. In these instances it would seem that the animals, brought down to the very lowest possible ebb of life, retain a sufficient reserve of oxygen to keep the flame of life alive. They are like animals hibernating in the extreme cold. The same phenomenon has been observed in the human subject in cases of exposure to lethal vapours in mines.

The idea that an atmosphere charged with five per cent. of carbonic oxide is instantly fatal to all warm-blooded animals is an entire fallacy. Some animals may be as rapidly affected, but others may continue to live a long time in an atmosphere containing at least five times that proportion of the gas.

These experiences have led me to increase the intensity of the lethal atmosphere far beyond what would, up to the present time, have been considered necessary. The period for induced sleep is by this means brought to the time that is required for the rapid narcotization of the human subject by chloroform, and the period on

to death is about a repetition of the same. By a further concentration of gas a more immediate effect, independently of all constitutional peculiarity and vital tenacity, might perhaps be secured; but then the death would partake more of the nature of a shock than of a sleep induced with easy and comparative slowness.

The atmosphere of the lethal chamber is not merely anæsthetic; it is also antiseptic. The dead animals can, therefore, be preserved in it, if required, while awaiting removal.

The whole arrangement is so carried out that the men employed are in no way exposed to the lethal action of the chamber. In order to be sure of this fact, I have placed myself with the men during all the preliminary investigations recorded in the above narrative.

On many other points of detail I shall have occasion to treat in future essays. It is sufficient to-day to place on record the first steps of a process which will, through science, give the boon of painless death to all the inferior animals that have ministered to our necessities, labours, or delights.

## COTEMPORARY PRACTICE AND LITERATURE.

*“ Every physician will and ought to make observations from his own experience ; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together.”—FRIEND.*

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### SMALL POX, CHICKEN POX, COW POX, AND VACCINATION.

**B**Y far the most important medical work for the past quarter is the report on the above-named subjects, by Joseph Jones, M.D., President of the Board of Health of the State of Louisiana.

The report consists of 410 closely-printed pages, and like everything that comes from the pen of this most original, industrious, and truly gifted author, is a model from which everyone who reads it may take something for copy and instruction.

Reading the essay carefully through, I am brought to the conclusion that no approach to it as a history of vaccination can be found elsewhere, and that we, in England, have nowhere collected in any volume anything like the amount of information that has been here produced by our learned American *confrère*. Every point connected with small pox, vaccination, and spurious



vaccination has been sought out, condensed, analysed ; while drawings, very natural in character, are freely interspersed to illustrate, from point to point, the author's histories, views, or conclusions.

Amongst the general conclusions which the author draws at the close of his treatise the following are some of the most important :—(a) Vaccination, when carefully performed on Jenner's method, is as complete a protection from small pox now as it was in the early part of the century ; (b) Without vaccination the application of steam and navigation and land travel would have, during the past fifty years, scattered small pox in every part of the habitable globe ; (c) Vaccination has not impaired the strength and vigour of the human race, but has added vastly to the sum of human life, happiness, and health ; (d) Inoculation for small pox, which preceded vaccination, induced a comparatively mild and protective disease, but multiplied the foci of contagion, kept small pox perpetually alive, and increased its fatal ravages amongst mankind.

On one subject only could it be wished that this excellent authority had bestowed more labour—namely, whether the diffusion of small pox in last century by the process of inoculation did not form the background on which vaccination stands so prominently as a blessing, not altogether unalloyed, to our race. In other words, if there had been no universal distribution of small pox by inoculation, would the disease under an improved hygiene have died out altogether without vaccination ? There is, perhaps, no one living who could answer this one all-important question more completely than our author. Will he undertake the task ?

## DISEASES OF MONKEYS.

**M**R. J. B. SUTTON, the Lecturer on Comparative Anatomy at Middlesex Hospital, contributes a paper to the *Proceedings of the Zoological Society of London* (Part I., for 1883, Longmans, pp. 581-586), which will tend to remove some very deep-rooted beliefs respecting the diseases to which monkeys are subjected in their exiled homes in this country. The common notion is that monkeys brought to England nearly all die from tuberculosis, an opinion freely endorsed by the profession of medicine.

After careful examination Mr. Sutton fails to find any reasonable excuse for such a wide-spread error.

Dr. Percy, in his account of "Management of Monkeys in Confinement" (a singularly interesting paper, by the way, published in the *Proceedings of the Zoological Society*, in 1845), named some of the diseases to which monkeys are liable. In one only did he find *phthisis pulmonalis*, and that was in a rhesus, bought from an itinerant showman. Dr. Crisp reported upon sixty-seven inspections of the quadrumana with three cases only of tubercle. In 1881 the Pathological Society, at the instance of Mr. Jonathan Hutchinson, appointed a committee to report on the present state of our knowledge on the diseases of the lower animals, and through Professor Flower, Mr. Sutton, as one of the committee, has had opportunities of investigating comparative pathology in all varieties of animals dying in the Zoological Gardens. From December 1st, 1881, to March 30th, 1883, an interval of sixteen months, 110 quadrumana of various species died. Of this number Sutton examined the viscera of 93.

*Diseases causing Death in ninety-three Cases of Monkeys in the Zoological Gardens.*

1. *Tubercle*, 3 deaths; 2 in a Rhesus Monkey, 1 in a Vervet, all Old World species.

2. *Bronchitis*, 22 deaths. Slight attacks are rapidly fatal.

3. *Pneumonia*. Lobar form 3 deaths; lobular, 7. The prevalence of lobular pneumonia results from the frequency of bronchitis and rickets.

4. *Empyema*, 2 cases.

5. *Abscess of lung*, bursting into a bronchus, 1 case, a Baboon.

6. *Œdema of lung*, 1 case, a Squirrel Monkey.

7. *Alveolar abscess and septic pneumonia*, 1 case, a young Chimpanzee.

8. *Scrofula*, 3 cases; a Baboon, a Capuchin, a Rhesus.

9. *Intussusception of the jejunum*, 1 case, in a fine Lemur.

10. *Leucocythemia*, with spleen enlarged to fifty times its usual size; 1 case, in a Lemur. The proportion of leucocytes in the blood 1 to 80 red corpuscles. This is interesting, as Lemurs come from Madagascar, an island "famous for ague." Lemurs are very liable to cataract. One Lemur died from purulent pericarditis, due to perforation of the pericardium by a caseating lymphatic gland.

11. *Typhoid fever*, 4 cases; 3 Lemurs and 1 Monkey. Two of the Lemurs lived in the same cage, and one seemed clearly to contract the disease from the other.

12. *Rickets*, 1 typical case occurring unexpectedly.

13. *The milk-white patch*, a thickening of the visceral layer of the pericardium; 1 case, in a young Rhea affected with rickets.

Upon these fatal cases from the different diseases named, Mr. Sutton makes a few comments of much scientific and practical value. In respect to one pathological point in typhoid affecting monkeys he shows that the mucous membrane of the intestine was ulcerated. Profiting by this experience, Sutton has now examined the condition of the lower end of the alimentary canal in the human subject after fatal typhoid, and has found in some cases a ring of infiltration and ulceration about half an inch above the anus. The explanation is not far to seek. Half an inch above the anus there is an abrupt change in the mucous membrane, the squamous epithelium of the anus being suddenly replaced by the columnar variety, beneath which is a substratum of lymphoid and glandular tissue. "It is this ring of tissue which in typhoid induces infiltration and ulceration, as does a Peyer's patch or solitary follicle in the ileum."

A better bit of inductive pathology than the above is not often met with.

The description of the phenomena of rickets in monkeys is excellently rendered. The chief symptoms, briefly told, are, "deformity, pain, paraplegia, incontinence of urine and fæces." The bones are enlarged and so soft that they can be cut with a knife as easily as a potato. Two different changes take place in the bones of the skull. In most monkeys the cranial bones are thinned so as to give rise to actual perforation. In others the roof of the skull is thickened to as much as half an inch in thickness. "The principal cause of rickets in monkeys is the fact that many of them are caught young, and in lieu of the breast milk of the mother, are fed on fruits, rice, and cows' milk."

The milk-white patch of the visceral layer of the

pericardium in the monkey is found, when it occurs, on various parts of the heart, and affords, Mr. Sutton thinks, support to the attrition or pressure theory of origin rather than to that of chronic inflammation. We must all hope that Mr. Sutton will continue his admirable work.

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METHYLENE BICHLORIDE.

**M**R. W. ROGER WILLIAMS, in a series of papers on the history of anæsthetics published in the *Medical Press and Circular* in May last, briefly condenses the subject, and introduces three names, those of Drs. Wilhite, Long, and Marey, in connection with the original discovery of administration of ether, names which have hitherto escaped notice. On the whole, he assigns to Morton, after Horace Wells, the chiefest credit. In speaking about the introduction of chloroform he very properly gives Waldie the honour due to him as the suggestor of chloroform to Simpson, while he omits to state that, in the form of chloric ether, chloroform was actually in use in London before Waldie named it to his friend, and that, in fact, he so named it because he was conversant with its use, and attributed the effects produced by chloric ether to its presence.

On the subject of bichloride of methylene Mr. Roger Williams falls into some important errors. He speaks of it as having been "of late" extensively used by Sir Spencer Wells. In fact, I administered it in the first instance, in 1867, to a patient of Sir Spencer Wells' for an operation performed by him, and he (Wells) has used it ever since that time all but exclusively. Mr. Roger



Williams speaks of the bichloride as a "mixture of some sort." The bichloride of methylene, as I introduced it, is a substance sufficiently definite, having a composition of  $\text{CH}_2\text{Cl}_2$ , a vapour density of 42, a boiling point of  $106^\circ \text{F.}$ , and a light pungent odour in vapour. It narcotizes with the greatest steadiness, but more readily than chloroform; and while belonging, as I have admitted throughout, to a dangerous family of chemical substances, it is, I believe, from its comparative lightness, safer than chloroform, but probably less safe than ether.

Of the fatality from methylene referred to by Mr. Williams it is difficult to form an estimate, because it is not known how many times it has been administered. I once saw a dentist administer it fifteen times in one hour, to different patients, for tooth extraction. For short operations it is often used in this rapid manner; and as it has been in use for sixteen years, it has had an extensive trial, attended with a reported mortality of ten deaths. In two instances of death, however, a specimen of the fluid used was sent to me, and proved to be absolute chloroform. In another case a mixture had been used of methylene with some form of ether. But even with these qualifying explanations the mortality from methylene bichloride is regrettable; and the further fact must be admitted, that to manufacture it on a large scale, and at as low a cost as that of ether and chloroform, is exceedingly difficult, and may, possibly, although I do not know the fact, have led to the introduction of compounds assuming to be methylene bichloride. Obviously, the fluid of which Mr. Williams speaks as having yielded seven parts of chloroform, and one of methylic alcohol, had nothing to do, if the analysis were correct, with the

bichloride. Obviously, too, some experimental results, to which he does not allude, from freshly-made bichloride, as it was supposed, had nothing to do with the true agent, but were due to the action of some compound containing free chlorine.

While I am writing these observations the *Lancet* of June 28th, 1884, p. 1171, records a death during the administration of the bichloride of methylene. The patient, Mrs. Jackson, daughter of Dr. Hammond, of Preston, and aged only twenty-three years, was about to undergo a slight operation by Dr. Aveling. She was in considerable dread of the procedure, and begged that Mr. Barwell, whom she had long known, should also be present. Mr. Fenton Jones proceeded to administer the bichloride by Junker's method. "In less than three minutes,"—I am quoting the report literally,—“and when only about ten minims of the anæsthetic had been used, the heart suddenly ceased to beat, and the respiration stopped.” The most prompt methods of restoration utterly failed.

It is most painful to commit to paper a record such as this, and I wish to make no apologies for methylene bichloride. It is at the same time only fair to say that here methylene was not the sole cause of death. Ten minims, forty minims, sixty minims of methylene delivered from Junker's inhaler, or in any form by inhalation, would not alone cause death. The lady who succumbed was, unfortunately, one of the *morituri* to whom reference was made in last *Asclepiad*. She fell into syncope from vital fear, and the worst that can be urged against the methylene is that it, perhaps, converted an ordinary into a fatal syncope. Such cases of premonitory fear are not uncommon. The late Sir William Fergusson once consulted me respecting a

fine stalwart man, under forty years of age and of lymphatic temperament, who being about to undergo an operation, by Sir William, for removal of a loose tumour in the ball of the thumb, was under the direst apprehension as to the effect of general anæsthesia. The impression made on the mind of Fergusson was so unfavourable that he sent the patient to me, as was occasionally his custom, to ascertain if I could detect any physical reason prohibiting anæsthesia. I could not. But I returned the opinion that the mental state of the patient might probably cause danger during the first stage of general anæsthesia, and that the local method by ether spray ought to be carried out. The advice was acted upon. I produced the local anæsthesia with great rapidity, and the tumour being unattached, Fergusson rolled it out with such dexterity that the whole operation did not last more than two minutes, anæsthesia included. We were both so intent on our work, we did not notice the patient until we saw his arm slipping from the table. We then looked up to find him in a state of deathly syncope. He was quite unconscious, his pulse had stopped, and for a short time we both thought that life was extinct; but laying him down with his head low and removing all impediment for free breathing, we had the satisfaction to see him recover.

After recovery this patient informed us that he knew nothing of the operation at all, but that the noise of the spray and the glitter of the knife in Sir William's hand turned him over in spite of his best resolution bravely to hold up. This patient was an instance of the *morituri*. Had chloroform, ether, methylene, or nitrous oxide been administered to him, the recoverable syncope would have been the irrecoverable, and I doubt if any general anæsthetic whatever will be discovered

free of this source of danger, unless we can introduce a new principle that shall neutralize the physical effects of fear.

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*SLEEP-WALKING AND HYPNOTISM.\**



MOST touching dedication to William Samuel Tuke introduces a book on the above subject by Dr. D. Hack Tuke, the father of the promising scholar to whom the dedication is offered. In this treatise the learned author points out that there is a parallelism between the phenomena of spontaneous somnambulism and those witnessed in hypnotism. In order to throw more light on this subject Dr. Tuke submits a series of questions respecting the phenomena of sleep-walking, to which he requests answers either by the sleep-walker himself or by others. These questions, twenty-five in number, are extremely practical, and properly answered, would afford much valuable information.

In regard to the cause of sleep-walking, Dr. Tuke observes, that although the evidence at command is not conclusive, we may regard the relationship between decidedly well-pronounced somnambulism and other nervous affections as fairly intimate, and hold that the neurotic constitution is a predisposing cause of sleep-walking. Age is a powerful predisposing cause of somnambulism, childhood and puberty being the periods at which the system is most liable to attacks of sleep-walking. The youngest patient Dr. Tuke has a note of was six years old, the oldest sixty-one. It is exceptional for the habit to continue after twenty.

\* J. & A. Churchill, London, 1884.

The reader will infer from the above that the learned author of "*Illustrations of the Influence of the Mind upon the Body in Health and Disease*" is once more offering to the world the fruits of his rich and varied experience.

WORKS ON THE DRINK QUESTION.

HE number of important works that have been published quite recently on the "Drink Question" indicate with sufficient precision the intense interest of the public mind on the whole subject.

"*Nineteen Centuries of Drink in England*,"\* written by the Rev. Dr. Valpy French, is one of these works, and if the discussion on strong drink had done no more than bring out such a volume, it need not be apologized for. Certainly a more interesting work it were difficult to find. Strong drink in all phases of English life and in all periods of English history; strong drink in its effects on men and on nations; strong drink in its influence on literature; strong drink with all its evils, perils, merriments, miseries, and devilries is here brought before us in a form and style which makes the book read like a paradoxical story, bacchanalian in its history, and anti-bacchanalian in its intention. The bibliographical chapter of *Nineteen Centuries of Drink* is of itself worth the price of a book which everyone should read.

"*The Foundation of Death: A Study of the Drink Question*,"† by Axel Gustafson, is another work which, for size and comprehensiveness of details, may well rank as a companion volume with "*Nineteen Centuries*."

\* Longmans, Green & Co., London, 1884.

† Kegan Paul, Trench & Co., London, 1884.



We may feel pretty sure that for a long time to come Mr. Gustafson's treatise will be a text book amongst temperance reformers. The different aspects of the drink question from the subject of drinking amongst the ancients, through physiological results, pathological results, social results, to the political social inquiry, "What can be done?" are considered in a lucid and striking descriptive detail and summary. In this, as in Dr. French's volume, a splendid bibliography, itself a study, completes the labour.

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#### *SUTURE OF NERVES.*

HE report which has just appeared to the effect that M. Tillaux has communicated to the Academy of Sciences the successful suture of nerve in two cases, and that in one case function has been restored in a nerve divided for a period of fifteen years, is, if confirmed, one of the most important facts we have had presented to us in our day. The physiologist, not less than the surgeon, will be led to important work by this event, and fresh fields of inquiry relative to nerve conduction may open new and unexpected advances in the theory as well as the practice of our art.

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#### *NEW INVENTIONS.*

##### A NEW HYGROMETER FOR INDICATING RELATIVE HUMIDITY OF THE AIR.

HE air absorbs moisture according to its temperature; and the warmer the air is the more moisture it can contain. Thus, air at a temperature of 80° F. can absorb as much as 10.9 grains of vapour in each cubic foot; while at a temperature of 50° it can only hold 4.1 grains, and at

32° only 2·1 grains. When the air has absorbed all the moisture it is capable of containing at its temperature, it is then termed “saturated.”

Usually the air is not saturated, but only contains a part of the moisture which at its existing temperature it could hold in suspension ; and that part is, in ordinary practice, expressed in a percentage as the “Relative Humidity,” or moisture of the air. For instance, if the temperature of the air were 80° F., and the relative humidity 50° F., this would show that while the air, at a temperature of 80°, could, if fully saturated, contain 10·9 grains of vapour in each cubic foot, it only holds half that amount—viz., 5·5 grains. This latter quantity, 5·5 grains, is the utmost moisture which the air is capable of containing at a temperature of 58·5° F. ; the air, therefore, would have to be cooled down to a temperature of 58·5° before it would be completely saturated with moisture—dew-point.

I am indebted to Mr. C. R. Schomberg, of Berlin, for the receipt of a very beautiful Hygrometer—invented by Mr. Lambrecht, after Saussure’s plan, and introduced into England by Messrs. Casella—of Holborn, by which the relative humidity of the atmosphere is indicated by a needle on a dial plate. A series of hairs arranged in a bundle, and sensitively hygrometric, produce the changes by which the needle is moved. The Hygrometer is so sensitive that the moisture of a single breath is sufficient to create a variation of the needle.





## MENTAL PHENOMENA OF TYPHOID OR ENTERIC FEVER.

*A SUGGESTION FOR DIAGNOSIS.*

ON the evening of June the 3rd, 1881, my friend Mr. Naughtin, of Baker Street, Portman Square, called me in consultation to Mr. Mulgrave Heaphy, civil engineer, who, in Mr. Naughtin's opinion, was lying in a very critical and peculiar condition of disease. I found Mr. Heaphy exceedingly depressed physically, but in an excited semi-delirious state of mind. In a short time he became collected, and then told me that he had been ill from the 16th of May, fixing the date with perfect precision from a particular circumstance of a painful character, which left no doubt as to the time when he first experienced a divergence from his usual good health.

Subjectively, Mr. Heaphy made far more of his mental than of his physical symptoms. He detailed his mental disturbance, which will by-and-bye be given in his own words, with great care, and this was his chief complaint. He added that from the first his appetite had been capricious and "strange," that sleep had been treacherous and dreamy, the least external sound

rousing him and causing much irritation and excitement. He had suffered some pain in the back, and a feeling, at times, as if he had been beaten; he had suffered also severe pain in the head, both sets of pain being in his view rheumatic or neuralgic. Until the day of my visit he had experienced little thirst, but now his mouth felt dry, and his tongue parched.

From the 16th of May he had been much depressed in physical power and uncertain of mind. He reported that a few hours before my visit he had been seized with an irrepressible desire to go to Brighton, and, accompanied by his mother, had been there; that they went to the Aquarium; that he essayed to take some food, but as soon as he began to partake of soup he was nauseated; that he instantly determined to return to London, and did return without a moment's delay. He had no business calling him to Brighton, no pleasure in going, and no definite reason for going except a desire for fresh air. He did not think the journey had done him harm, but something had brought on intense headache, parched his tongue, and excited his mind.

The objective symptoms were acute. The temperature was raised to  $104^{\circ}$  Fah. The pulse was sharp and irregular, ranging from 100 to 130 strokes per minute. The heart sounds were clear and sharp. The respirations were hurried, from 35 to 40 per minute, but there was no crepitation, nor bronchial rale, nor bronchial obstruction. The skin was dry and burning, but entirely free of rash of any kind. The tongue was dry, slightly coated, cross-chapped and sore. The bowels had acted twice in the day, and the matter last passed was loose, hæmorrhagic, and offensive; but there was no tympanitis and no abdominal tenderness on pressure. The expression of the face was that of extreme cerebral pain and

almost of terror ; the brow was knitted, the look wild and anxious, the head frequently tossed from pillow to pillow ; all movements, slow at first, were afterwards rapid, prolonged, and, as it seemed, automatic. In consultation with Mr. Naughtin, we discussed whether we had before us a case of typhoid, or one of acute cerebral meningitis, what the old practitioners would have called "brain fever." Presuming that the disease were typhoid, and the patient strictly correct in assigning the 16th of May as the date of commencement, then on this day, June the 3rd, full eighteen days after the commencement of the attack, the rash ought to have been present, and the abdominal disturbance ought long before to have declared itself. Moreover, in searching back to the cause, there ought to have been found some exposure to contagion ; and this was not found, while a sufficient cause for severest mental shock was clear enough. We inclined, therefore, to the idea of cerebral meningeal inflammation, but determined at the same time to keep the closest look-out for typhoid rash and for other symptoms of that affection.

On the following day, June 4th, the symptoms remained generally the same, but with increasing diarrhœa without rash. The next day I did not visit, but Mr. Naughtin did, three times, and found no very important change. On the 6th of June, on making his first or morning call, Mr. Naughtin sent for me, to observe with him that a distinctive typhoid rash had become suddenly and generally developed, fullest on the abdomen. The disease was now clearly defined, although as to its origin no direct light relating to contagion had been thrown. The patient was now conveyed to the London Fever Hospital, where he passed through all



the ordinary stages of typhoid or enteric fever, and was discharged convalescent in the early part of July.

I write these brief notices of Mr. Heaphy's condition in order to prepare the reader for the very interesting record which is to follow from himself. While I was visiting him, in reply to my questions, he narrated to me, with singular earnestness, his mental sensations from the day when he first became ill, and this narrative so attracted me, that I essayed to write it down as a new chapter in the history of disease. On his discharge from the hospital he called upon me, and with great minuteness continued the report of his mental condition after he had been removed from my observation up to the period of completed convalescence.

Of this narrative I again took notes. Finally I did what was better still. I induced Mr. Heaphy to write out the description in his own words, explaining to him as a reason for the request a new line of research I had in my mind, namely, to obtain the natural mental shades of a definite physical disease. With much care he kindly drew up the following report.

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*ON THE EFFECT OF ENTERIC FEVER, DURING ITS COURSE,  
UPON THE MENTAL POWERS.*

"WHILST engaged, in May, 1881, upon some professional work in the north of England and Scotland, I contracted a very severe attack of typhoid fever, which showed itself shortly after my return to London.

*May 16th.*—The first mental symptom I had was that when a great difficulty presented itself, it appeared to my mind to be insurmountable; there was no mental spring or buoyant feeling to give me confidence that, in the end, I should be able to overcome the difficulty.

*May 26th.*—This was followed after nine days or so by utter loss of appetite, the thought of any kind of food producing a feeling of nausea. I began to have a peculiar sickly sensation in the intestines.

*May 27th to May 31st.*—The next mental symptom I became aware of was that I was losing the power of drawing a general conclusion from a number of varied facts to such a degree that I felt no confidence in the result I arrived at—I was writing a report upon some large works—without a feeling of timidity and great hesitation. I hesitated over conclusions that would have appeared self-evident to me before. My memory was clear and good. I could do any work that required ordinary reasoning power, provided it was that which I had been previously accustomed to. Had no solid food since the 25th. Took a Turkish bath, and perspired moderately whilst having it. Felt great nausea against food of every description except tea; a warm gnawing sensation in one particular spot in the intestines, and when the idea of food came into my mind a heightened sensibility in that particular spot. The size of the spot seemed to be as large as a sixpence.

*May 31st to June 8th.*—In the next stage all power of work had left me. I shrank from everything requiring an effort of thought, but I was highly emotional, with a tender regard for all about me, accompanied by a feeling of wishing to give, money appearing to be of no value. All power to check the emotions was gone. I had intervals of delirium, retched a good deal but brought nothing up, and had diarrhoea. My tongue was ulcerated, the loathing against food of every description was intense, a thirst was upon me that nothing could quench, and a most singular and strange bodily sensation pervaded me at times: it was as if the whole of my

blood and muscles were infested with trichinæ, and that all the corpuscles in my blood were in a state of tingling irritability, and that they had the power of making their sense of discomfort apparent to me. I had no delirium while this sensation was upon me; on the contrary, I read a little of Knight's "Pictorial History of England" and Kingsley's "Hypatia," and remember now what I read. I may mention that on June 3rd, I got up, walked about half a mile, then took the Pullman train to Brighton, walked from the station there to the Aquarium, but felt so ill that I took a conveyance back to the station, and came by the next train to London. Hemorrhagic diarrhœa set in a few hours after, and continued for several days, being sometimes profuse. The reason I went to Brighton was through an irresistible desire for cool fresh air. The rose spots showed themselves on June 6th.

*June 8th to June 10th.*—In the next stage the mental power I seemed to possess was little more than a consciousness of what was passing around me, and of my own bodily discomfort. My mind with regard to its activity might be likened to that of some cold-blooded animal, yet when aroused by a question my memory upon the matter asked was perfectly clear and good. The fear of death did not at all trouble me. I was deaf. My emaciation and weakness were extreme. My voice was completely changed. I lay with my hands crossed in front of me, and could barely raise them to my face. No delirium, but bordering upon it at times.

*June 10th to June 12th.*—In the stage after this I can liken myself to nothing much better than a pulsating invertebrate creature, and this was accompanied at times by a curious mental state. I felt as if the princi-

pal seat of intelligence was in my body, and that it was able to perform its functions without any help from the brain. The nerve centres or ganglions in or about my intestines appeared to possess consciousness, and my brain was aware of it. I believe it was during this period that I was in a state of such extreme prostration as to be hovering between life and death. My brain to everything external was passive,—unless specially aroused; it seemed to be aware that any activity would be at the expense of the vital centres, but at the same time there was a kind of intelligent communication going on between it and them; each could feel what the other was thinking of. I use the word “feel,” for I have none other by which I can so well express the way the communications passed, although the word does not adequately describe what I should like to explain. My brain told my ganglions that everything depended upon them, and that it must leave all to them; the ganglions seemed to be aware of it; they appeared to be in a great state of exaltation and quite conscious of what course they had to adopt, and that they had had previous experience of how to act. My mind was at rest, with a sense of calm upon it that I had left everything to them.

The next thing I forcibly remember was waking up, after a long sleep, in a state of profuse perspiration, more especially about the abdomen, accompanied by a tingling feeling of life there and with an intense desire to rub and scratch. Then I knew I had taken a turn and should recover—something told me that I had lived through the disease. Although from this time no further conscious messages, that I can distinctly remember, passed between my brain and the ganglions, yet I seemed to feel that they were still actively at

work, and the mental trust I had, as it were, placed in them continued.

*June 12th to June 18th.*—I had nothing more to do for some days but to rest as quietly as it was possible for me to do. I was very deaf; I could not read, and could speak but a few words at a time. My mind was perfectly clear. I began now to experience a longing to pass the time away, and nature sent a relief. I found that I was capable, at an effort of the will, to transport myself back to any opera or theatre I had been to within the last year or two, and sit out the performance again. The impressions produced were as real as if I had actually been present at the performances (I was taking no opium). I listened to the music, could see the performers act and hear them sing and talk; almost every movement of their bodies came back to me. So real was it that with regard to the opera of *Carmen* and the opera bouffe of *Madame Favart*, the faces and backs of the audiences, as I last saw them, came before me again. This power was perfectly under my control. I could throw off the impressions at any time, as, for instance, when I wished to take nourishment, or talk to the nurses or doctors, after which I could at once resume them.

I was now getting into the emotional stage again, but the emotions were not quite of the same character as those which had occurred previously. I was more selfish. I longed for people to come to me, to bring me flowers, to give me things, and tried to coax the nurses in an emotional way to get what I wanted. In the previous emotional stage the feeling upon me was one of wishing to be the means of helping and aiding others, or giving them happiness; now I wished others to attend to my feelings and wants, as if they were of the first and almost only consequence. The emotions were



those of a child. A longing for solid food was upon me. I began to read on the 17th, and it is impossible to describe the relief and pleasure it afforded me.

*June 18th.*—The sensations of hunger that now took possession of me were irresistible ; under their influence my mental state with regard to food was that of a savage or a carnivorous wild beast ; the idea of food would overwhelm every other thought. If I could have crawled out of bed, I felt as if I could almost have strangled anybody to get meat, though I should not, of course, have attempted such a thing, as my will would have prevented it even if I had had the strength. I had an intense desire to seize a piece of meat, crawl into bed with it, hide under the clothes, and tear the meat whilst I held it in my hands. I may add that for some time after I did have meat, I used to watch that nobody was looking, and would then take up a piece of bone with meat upon it off my plate and tear the meat away with my teeth. I never cared to pick a bone before my illness, neither do I now that it is all over. From what I have experienced, I believe that many persons suffering from hunger would find it utterly beyond their control to prevent themselves from taking food, no matter how they tried. Should I ever be on a jury and such a case be brought before me, I never would convict, as I can quite understand a person being at the time irresponsible.

*July 1st (about).*—In my next mental stage a feeling of youthfulness and gaiety was upon me ; the future took a picturesque form. I had scarcely a care. I enjoyed each day as it went, and once again I experienced that most enviable of all states, that of perfectly living again in the present. This was followed by keen activity of all my mental faculties.

It is now a year since I was attacked. I have not yet recovered all my old strength. I have still at times pains down my right leg. My mind from the long rest is singularly clear and youthful: my mental powers are as strong as they ever have been in my life, but I shrink from anything entailing worry, and I have little inclination to turn, after my day's work is over, to any other matter that requires much mental exertion.

I was in my 38th year when attacked.

MUSGRAVE HEAPHY, C.E., F.G.S.

May, 1882.

#### *SUGGESTIVE COMMENTARY.*

IN reviewing the details of the above report,—so originally and admirably told,—apart from various practical suggestions which will strike the mind of the physician, there are some general facts, which to every thoughtful member of the reading community will prove of permanent as well as of current importance.

The fact that the function of memory remained perfect during changes of mind departing so largely from what is common, will not be passed over. Memory, the storehouse of the senses, often retains, in this same way, its function through acute disease, and may, perhaps, when so retained, be accepted as an indication that the higher or intellectual nervous centres are unaffected. In every disease, acute or chronic, the condition of memory is, therefore, of immense significance. It is remarkable as a prognostic indication, for good when it is perfect, for bad when it is imperfect.

Another phenomenon of interest is the facility with

which the patient was able, at one stage of his illness, to recall events of his past life which had been to him of a peculiarly pleasing or intellectual character, and which now, by mere recall, seemed to absorb and include, for a time, all his existence. This state, an exaltation of memory on particular subjects, is not infrequent during many diseases in which extreme exhaustion is the marked characteristic. The exaltation may even extend to the dying, and so extending may account for many of the statements of impressions made by them, which are sometimes accepted as of supernatural origin.

The most curious part of the narrative in its personal as well as general bearings, relates to that irrepressible, devouring desire for food, which attended the early stage of convalescence. Mr. Heaphy's notes have been in my hands for two years; but if they had been written afresh, apropos to the sufferings and acts of the survivors of the crew of the *Mignonette*, they could not have taught more eloquently the quality of mercy towards such unfortunates.

### *Cause of the Mental Aberrations.*

What was the cause of these peculiarities of mental phenomena incident to the disease? Suppose we say that the phenomena were due to a disturbance of the balance between the intellectual and vital nervous centres. In the use of such an exposition we have hold of something that is near to the truth, if not the whole truth. Mr. Heaphy describes phenomena, felt in the commencement of his case, which indicate as a primary part of the disease a surface irritation extending to the vital nervous centres. That irritation led to failure of the higher nervous centres and to inability for mental

work as a necessary consequence, the circulation through them and the nutrition of them being disturbed and reduced. The inference therefore, and the inference is borne out by the general history of typhoid, is that the primary seats of the mental derangements are the ganglionic nervous centres. They are first excited, then exhausted, and throughout are thrown into irregular action, the changes induced extending through the communications of the nervous cords emanating from them to the whole of the vascular network to which they extend. The symptoms which follow,—symptoms of deranged nutrition, of deranged circulation, of surface congestion, of febrile heat, of discharge and passive hæmorrhage from the bowels, of ulceration in the intestinal tract, of eruption in some of the areas of nervous supply on the skin, of dry tongue, and of arrested digestion,—these physical symptoms, as well as those which are mental, all point, as with one indicator, to the first or primary seat of derangement in typhoid or enteric fever.

So far this is satisfactory, but what is the nature of the primary injury to the nervous centres? There may be four modes by which the centres are injured:—

1. By exhaustion, consequent upon absolute or partial deprivation of food, while they are subjected to great activity.
2. By direct shock, physical or mental.
3. By diffusion through them of some volatile diffusible organic agent.
4. By the influence of agents taken into the body which exhaust the vital centres by causing an exhaustive irritation in, and a profuse discharge or hæmorrhage from the excreting surfaces.

In different classes of disease we see different of these causes at work. In famine or remittent fever the first-named cause is pre-eminent and obvious. In syncope, in flushing and excitement, in hysterical excitement, in many febrile states, continued and remitted, under which some persons suffer for months and even years without being laid up as sick, we see, constantly, the second-named class of causes acting singly or combined with the first;—physical or mental strain or shock combined with irregular feeding and improper diet.

In typhus and other acute affections, attended with delirious dreams, like those caused by some anæsthetics, we see the third-named class of causes pre-eminently active.

In every case of extreme irritation of a sensitive secerning surface followed by excessive secretion, we see the action of the fourth-named class of causes.

In observation on the effect of any of these sets of promoting influences we witness some mental aberrations, such aberrations as, modified to the possibility of remaining persistent, lapse, sometimes, into various phases of what is called insanity.

In Mr. Heaphy's case, I believe that the second of the classes of causes named, aided by the irregular mode of sustenance referred to in the first class, were the absolute promoting causes of his febrile seizure. In brief, I consider his enteric fever to have been of purely nervous origin, not spontaneous, because external influences affecting the nervous system played their part, but of individual origin, *i.e.*, within his own organization; and although in the expression of this view I must run counter to those who assign to every coincidence of finding minute animal or vegeta-



ble forms in diseased persons a first cause of the disease, I am not afraid of the contest in the long run.

*Ideas as to Remedial Measures.*

PRESUMING that there is sound reason for the suspicion that the primary seat of the enteric fever was in the vital nervous system, there is much that suggests itself in respect to treatment. Under this view all treatment as yet practised is directed to secondary changes, while the original causes remain untouched. Such treatment is like throwing cold water on the heated axle of a railway carriage while the engine continues to pull it on at full speed. It can only be partly useful. The direction of thought should be towards the best method of restoring the vital centres. At present we know of but one method towards this end, namely, absolute mental and bodily rest. Under that the cerebral, the sensory, and the spinal centres cease to have any severe demands made upon them, so that what force is produced from food and air goes chiefly to the vital centres under which they may, slowly and by marked stages, recover tone. But we ought not to stop at this method. It is very ignorant animal engineering not to know how to feed the nervous system, through some channel into it, by a quicker and surer process than this expectant method, to wait for which is too often to let the patient succumb from the irrepressible secondary or sequential complications. There is a chapter of treatment yet to be written on this subject which will be of signal intelligence and usefulness.

*Diagnostic interpretations.*

For the moment the most pressing consideration springing out of the record that has been given above re-

lates to the diagnostic interpretations which may be obtained from it. Is it possible that each well-defined physical disease is accompanied by its own equally well-defined mental phenomena? I suspect that is the fact, and that perhaps my patient has written the first subjective essay on this line of discovery. He, by a most intelligent analysis of varying classes of nervous phenomena, has indicated the vital, emotional, and intellectual aberrations of one disease.

In the first instance, the failure he felt was in the purely vital organization. He lost appetite, and he lost what he most appropriately calls "mental spring," soon after which there followed the sickly sensation having its seat in the intestinal canal.

In the next instance, the failure of the primary vital power led to a further and deeper failure of the volitional. He was not able to will according to his natural capacity. At the same time his emotional faculties were disturbed, and his vital functions were more completely deranged.

In a third stage, when the fever was actually pronounced, his higher or intellectual faculties were held partly in abeyance; his "mind might be likened to that of a cold-blooded animal," memory lying behind intact, but one sense, that of hearing, impaired.

In a fourth stage, the intellectual centres seem to have been almost entirely in abeyance, the vital alone active, so that the subject of this condition had an experience as if he were living by the vital centres alone. This was his period of extremest prostration, when he was, as he says, "hovering between life and death," the rally which succeeded being accompanied by a reaction over the cutaneous surface with subsequent surface-tingling and irritation. This, I doubt not, was

due to return of circulation in the vessels of the minute circulation of the skin, for I have repeatedly felt the same passive irritation, locally, during and after reaction, when I have frozen a part of my body with ether spray. The same sensation is also commonly felt in wounds that are nearly healed, and has given origin to the common knowledge that a wound is coming near to recovery when "it itches."

In a fifth stage, the general reaction from passivity of mind was foremost and most intense in the vital or animal centres. Then followed the re-development of intellectual capacity with the recurrence, rapidly, of apparent evolution from childhood into manhood, accompanied by the caprice incident to the influence of desire or appetite, but controlled by some degree of reason, and expressed by emotion.

Lastly, there was the return of nervous balance, and upon that return the sensations of renewed strength and activity which characterize the time of youth, not probably representing anything like an actual renewal of adolescence, but, by comparison with the previous prostration, a transient, yet distinctive feeling, of such renewal.

It is exceedingly rare for the physician to have under his care a patient who can write down, as Mr. Heaphy has written, the sensations which he experienced stage by stage. I can, however, say that the phenomena he has described tally well with the statements which other sufferers have made to me during the differing stages of enteric fever, and do, I believe, fairly map out the mental diagnoses of that particular affection. Typhus has a different mental registry; remittent fever a different; scarlet fever a different still, as I know, personally, from subjective as well as objective ex-

perience. Typhus is marked by short, delirious, broken dreams. Remittent fever by long, dolorous, painful dreams. Scarlet fever by realistic dreams, influenced or excited by immediate environments. When I had scarlet fever, one of these realistic dreams, occasioned by something that was going on in the room, and which appealed to the olfactory sense, was so absurd that I cannot suppress a smile, thirty-four years afterwards, when I think of it.

In the subject broached in this paper, a road is opened by working in which any industrious and observant student of medicine may win a golden harvest, brightened by the hope of usefulness and reputation. Toiling in this field, he must afford to wait patiently for the acknowledgment of his labours, assured, indeed, from the outset, that it will be many a long year before he will be understood, but equally assured that when understood the appreciation, superbly enviable, will repay every stretch of industry, however severe, however prolonged. My work must be accepted simply as an index finger pointing the way.

*A SPONTANEOUS CURE OF EXTREME  
PORTAL CONGESTION BY COPIOUS ACCI-  
DENTAL HÆMORRHAGE.*

*A CLINICAL STUDY.*

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**M**R. H. S., a well-known and distinguished bibliographer, aged sixty-five, of bilious-lymphatic temperament, of sedentary life, of height five feet nine, and of weight about two hundred and thirty pounds, consulted me in March of this present year concerning a series of important symptoms, affecting his alimentary and portal systems. The one chief and distressing symptom was extreme distention of the abdomen from accumulation of flatus. The body, ordinarily, some forty-eight inches of girth at the umbilicus, was so distended that the clothing had to be increased quite six inches in size, to meet the inconvenience. The abdomen was tympanitic generally, and the distress from the tympanitis was extreme. The diaphragm was pressed upwards, and was so impeded in its action that the breathing was dangerously embarrassed, any slight motion of the body by a quick walk, or other movement, being quite sufficient to cause the patient to



pant for breath, and to stand still in order to recover his breath. In the recumbent position the breathing was so difficult, that a specially-constructed chair-bed had to be obtained in order to secure sleep, sleep being entirely prevented unless the sitting position were maintained, and being even then broken by any giving way of the cushions or pillows backwards.

The physical diagnostic signs of an objective character were not striking after the distention of the abdomen had been taken into account. Percussion over the abdomen indicated no dullness at any point, but the distention prevented the physical detection of disease in the abdominal organs. The stethoscope yielded no indication of aortic pulsation upon any enlarged or indurated viscus. The chest sounds, except for an occasional bronchial râle, the result of an old-standing bronchial difficulty, were natural, and the chest percussion was everywhere clear. The heart-sounds were natural; the cardiac impulse fair; the action, as a rule, regular, but disturbed when the distention was at its worst by an irregular, but not intermittent, motion. There was no sign of any cerebral or spinal mischief. The mind was at all times clear, the will good. The spirits were, occasionally, somewhat depressed, but, on the whole, cheerful and hopeful.

The urine was natural in respect both to quantity and to quality.

The tongue was generally clean; the appetite little impaired. The primary digestion was rapid and without discomfort, but the bowels were always constipated, and, unless aperients were regularly given, were loaded with hard and firm excretion, which was discharged with difficulty and pain. If the constipation were

permitted for so long as one day, the distention from flatus was dangerously increased. On the other hand, when the bowels were freely open, the relief obtained from the flatulency was, for the time, most marked. There was never, at any time, a sign of jaundice.

Looking upon the case as one of extreme congestion of the hepatic and portal systems, and judging, from the hard mental life of the patient, that the congestion was probably due to deficient nervous control in the vital nervous system, I prescribed accordingly. The most rigid attention was paid to diet. Foods and drinks which seemed in any degree to increase the distention were excluded. Tea, coffee, and alcohol were excluded for this reason, together with all varieties of pastry and pulses. Care was taken that the quantity of food consumed at each meal should be most moderate. Solid food was brought to the finest state of subdivision, and was eaten slowly after good mastication. The meals were carefully adjusted as to time. With meals containing albuminous substances, solution of pepsine and hydrochloric acid was combined, and a short time after every meal Benger's pancreatic solution was taken with bicarbonate of soda. The foods were all cooked with the utmost care.

Reviewing, in detail, my report on this head of dietary, I can recall nothing which I would wish, under similar circumstances, to revise.

Medicinally, I first prescribed mercurial alteratives at proper intervals, and as a regular medicine, tincture of *nux vomica*, with dilute nitric acid and *taraxacum*. This plan seeming, after a time, to yield no service, I ordered bismuth, bismuth with *nux*, bismuth with iron. I also ordered charcoal for the relief of the distention, and other means. I cannot say much in favour of

any of these remedies, except for Benger's pancreatic solution. When that was omitted, I have no doubt that the discomfort and distention were increased. The mercurial aperients produced good action of the bowels and excretion of bile, but they depressed, and did, ultimately, more harm than good.

In the end, as a matter of practice, the most successful plan was the administration of a dose of castor oil every day, so as to ensure at least one free alvine action. The oil was always effective; it gave no pain by its effects, and it lessened the distention more effectively than any other medicine. Its influence, however, was transient and merely mechanical. It relieved without affording any indication of an approach to cure. If it were omitted, there was invariably hard alvine accumulation, and all the distressing symptoms were very much intensified.

Respecting the flatulency and the removal of it, it may be remarked here that the relief obtained was sometimes by the bowels and sometimes by a long series of eructations which always, when they occurred, gave most relief. The patient himself described to me, over and over again, that to his sensation there were "two chambers" in the abdomen in which the gases were collected; one he described as the upper, the other as the lower chamber, the accumulation of gases commencing in the first and passing into the second. While the accumulation was in the upper chamber, free eructations gave great relief; when it had become transferred to the lower chamber, no eructation afforded the slightest relief. The description thus supplied suggested to my mind, and I think correctly, that the gases were first thrown out in the small intestines, distending them, and that they were then transferred

into the colon, causing it, in turn, to expand as their receptacle.

Matters continued unchanged for some weeks, and then a new symptom appeared in œdema of both feet, extending to the ankles. This was rapid and severe, and caused more difficulty still in walking and in recumbency. All the symptoms were, indeed, now assuming a most serious aspect.

At this critical juncture there occurred the important circumstance for which I relate this history, namely, an accidental natural cure by profuse hæmorrhage. One morning, during the action of the bowels, the patient passed, all at once, by the bowels, over a pint and a half of blood. The blood was of red colour, and coagulated in large clots. The loss caused no faintness, but afforded instant relief to the oppressive symptoms. The relief was such that Mr. S., whom I had been visiting daily at his own house, came to my consulting rooms, full two miles distant, to let me see him, and to report the change that had taken place. He was not alarmed, because on one or two previous occasions in his life blood had passed by his bowels, with relief to vertigo, from which he had suffered; but he was in the best spirits, because he could breathe freely again, and could lay himself back in a chair without experiencing oppression.

The bowels being still constipated, I recommended that a dose of castor oil should be taken early on the following morning, notwithstanding the loss of blood. The advice was followed, and during the action of the oil a full pint and a half of blood again escaped from the bowels. On the night of that day the patient could lie down completely straight, the œdema of the feet disappeared as rapidly as it had appeared, and from

that period good steady recovery set in. For a few days there was a little further loss of blood by the bowels, an ounce or two with each action; but that subsided, and the extreme distention also passed away.

On Saturday, July 26th, I made an excursion, for the day, out of town with my patient, he being in his usual health, and looking as well and active as I remember to have seen him for many years past.

#### PRACTICAL COMMENTARY.

In these days, when the practice of blood-letting is considered out of date, the history of a case like to the above is most important in a clinical point of view. Now that the danger is past, I may confidently state that, at the time immediately preceding the accidental hæmorrhage, which led to such excellent results, I had the most serious apprehensions for my friend and patient. I am also sure that any other physician visiting a man of over middle age, who was unable to lie down for a minute because of the breathlessness following the act, and who had extreme abdominal tympany, combined with œdematous feet and legs from venous obstruction in the abdomen, would not have been less anxious than I was. The anxiety was intensified by the failure of all reasonable suggestion as to what more was to be done. Purging caused exhaustion without giving relief. To have used needle puncture for lessening the distention of the abdomen would have been a mere temporary expedient, wise perhaps in emergency, but hopeless as a means of cure.

At this juncture, so critical, came the spontaneous cure. By an accident, a blood vessel, probably one of the large hæmorrhoidal veins, gave way under the tension to which it was subjected, and the whole portal



circulation was instantly relieved. From this loss of blood, affording such immediate relief, the patient suffered no enfeeblement, because the blood which escaped was doing no work. It was obstructing without working. It probably was undergoing slow metamorphosis and yielding the gases which were the cause of the mechanical oppression and mischief.

Some would describe this hæmorrhage as a natural cure, and speak, in eulogy, of the *vis medicatrix naturæ*, as if some hidden power, called nature, had put forth, by intention, an intelligent hand at the proper time, and had done for art what art could not do for itself. I cannot take that view. Nature went her own way without any reference to the life or death of the patient. A little more of rupture of the vessels that yielded the blood, and the patient, instead of recovering, had bled to death. As it happened, the loss of blood incident to the pressure of blood was just as much as gave relief, and no more,—a fortunate result, and one likely to be fortunate in hæmorrhage from vessels in which the *vis à tergo* is partly broken and the direct impulse of the heart is wanting; but at best accidental, or, let us say, spontaneous. For all that, we ought to seize the practical lesson which such an accident affords us. Our forefathers—empirics! yes!—seized many such lessons and held them fast, and we might well follow their lead. Art is omnivorous. She learns, or should learn, from all sides and all occurrences. The spontaneous hæmorrhage, the loss of sixty ounces of blood in two days, saved my patient. That is the practical outcome of his case.

Some thirty years ago, I should, from empirical teaching, have bled that patient from the arm, or have cupped him over the abdomen, or have applied leeches

to the lower part of the alimentary tube. The general abstraction of blood from the arm, the local abstraction of it by the cupping glasses, would have taken away working blood, would have weakened the body; but I doubt whether it would have relieved the portal congestion, except it had been dangerously excessive. Would the old-fashioned plan of leeches to the rectum have been more directly beneficial? I should in a similar case give that method full effect, if permission were granted.

The cure in the case recorded is exceptional. The case itself is far from exceptional. In these days, when well-fed men live a close sedentary life, congestion of the circulation, commencing in the outlying suspended portal circulation, is very common. In them, structural evil of a secondary kind, and long unsuspected, has often its primary origin in the portal system. Such men, living often too well, bring their digestive organs up to the fullest development for hard digestive work, while the volume of their blood, which passes from the abdominal aorta to supply their digestive organs and to fill their subsidiary venous canals, is enormous. They are one part out of three chylopoetic. Let the portal flow be checked in them, and at once they are overthrown. Their hepatic function is impeded, their intestines are engorged; the secreted fluid from their cerebro-spinal canal finds difficult exit. So they become hypertrophied in visceral organs, sluggish in vital, drowsy in cerebral. They have laid the basis for the physical degenerations of senility, and they carry the fact so openly that their friends express it for them in the saying that "they are on their last legs." They often are.

By a change of regimen, a bold change it may be, by

change of diet, painful at first as well as bold it may be, art can come to the aid of these sufferers, and lead them to a longer and better life, if she can be allowed time to carry out her systematic curative plans. But often she is not allowed the time. Often a rapid obstruction frustrates fatally, or would do so if a spontaneous accident did not effect a rapid and safe cure for the more urgent distress. I have shown, as if I were writing a page of medicine that would have passed for clinical half a century ago, that this rapid and spontaneous cure may take place by copious accidental hæmorrhage, and I would—if I dared suggest so retrograde a course—recall, with nature as the ministering spirit, an old practical fact and a practice based upon it.

*A GREAT MEDICAL REFORMER,  
THOMAS WAKLEY, M.P.*

*A PERSONAL RECOLLECTION AND TRIBUTE.*

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**I**N previous numbers of the ASCLEPIAD I have written brief notices of some of those illustrious whose lives are precious for study.

To-day I sit down to write, while still the memory is fresh upon me, some recollections of a man who, when I was in the vestibule of the Esculapian Temple, was one of the most striking figures within it, and who, when I entered the temple, was amongst the first to give me welcome, to strengthen me in intention and inspire me with hope.

This man was not, like my Gilbert and Harvey, a discoverer in natural science; he was not, like my Keats, a poet. He had few of the qualities of any of those of "our great ones of the past," whose lives I wrote long ago. But in his way he was very great, very powerful, very much feared, and, withal, and as a consequence, very severely attacked by many, but by none more than by those who felt surest that he would never divine, by their manner and bearing towards him, from whom the offence came.

To my young mind it was a new experience to see the behaviour with which this man was treated, such merciless aspersion, such truckling servility, such cowardice, and often too from quarters where it was least expected. I remember talking to my good old friend, Dr. Robert Willis, on this subject, and saying that I had half a mind to leave a profession in which I was morally disheartened, and try my luck at the bar. "You will fare no better there," he replied, "if you try it, and you had better go on. As to the man you name, in whatever profession he had entered, he would have shared the same fortune and misfortune. In the Church he would have been a Pope or a martyr; in the Law a Lord Chancellor, or a reformer out of the recognized pale; in the Army a General or a rebel; in the Navy an Admiral or a corsair. In Medicine he has had no scope for such extremes, but he is what he is there, a character quite unique, utterly fearless, resentful of all narrow abuses, and, according to his lights, honest as he is fearless and determined; a man who would crush an enemy as he would a wasp, and treat a flatterer, whom he knew for such, as a born enemy; but who loves his friends and helps some, whom he thinks well of, much beyond their merits."

This was Willis' summary of his cotemporary, and Willis had every chance of being near the truth. He had himself just resigned the editorial chair of the *Medical Gazette*, and the cotemporary of whom he spoke, and whom I have on the easel now, was Thomas Wakley, M.P., Coroner for Middlesex and the founder and editor of the rival medical journal, *The Lancet*.

It seems to me but yesterday since I first saw this famous editor of *The Lancet*. He was in the zenith of his power. He had overcome most of his enemies,



and he was beginning, at last, not only to be admired for his abilities, and feared for his power, but liked for his nature, abounding, as it did, in qualities which attached, with a close attachment, all who were intimate with him. This phase of feeling towards him did not develop, however, from within the professional pale. It began from the outside, from the people; from the people when they discovered in the honourable member for Finsbury, and in the energetic and powerful coroner for Middlesex, as well as in the editor of *The Lancet*, a friend who stood up against every oppression, and who, at all risks, defended the oppressed against the most potent oppressors.

A soldier had been whipped to death in Middlesex. In these days it is somewhat awful to think that such a thing could have been; that a man, for any offence, could be led out into open day and deliberately "cut into shreds," until he died slowly and horribly. Yet, in the name of military law, this thing had been done; and, as a coroner holds still an inquest on a man who has been executed by the hangman, Mr. Wakley had to hold an inquest on this soldier executed by the lash. There was probably not one other coroner in England who would not have held that inquest from mere formal necessity, have instructed the jury that the death was an unfortunate natural accident of the punishment, and so have dismissed the case. The coroner for Middlesex viewed the matter with different eyes. To his clear mind the accident was murder. He made no secret of his views; he showed no hesitation as to his duty. The leaders of the army were against him on the plea of discipline; the legal authorities, some of the highest, on the plea of law; the "rag" of every club was against him on the score of danger to aristocratic stability;

the Government was against him on the score of difficulties which it would encounter in introducing a change of punishment in the service; the press was for and against him, but more against than for him, because the subject of change of law had not then been raised to such a pitch as to be a burning question, ripe for solution.

In spite of all, the coroner stood firm. There was no point of law on which he had not prepared himself; there was no medical, no surgical fact on which he did not obtain competent and weighty evidence. The result was that the exposures he made, and the verdict he obtained, rendered whipping to death ever after impossible in the army of Great Britain.

The reaction in favour of the coroner has to be remembered in order to be understood. In the House of Commons he had been winning his way. His bold, rough Saxon eloquence, which first became welcome to Finsbury, had been recognized in the Parliament. This audacious justice in the Middlesex coroner's court completed the triumph. It excited the popular heart wherever that heart beat, and the name of Mr. Wakley became a household word.

It was then that even opponents in the profession of medicine began to see that they had amongst them a man. Whether they loved him more, or feared him less, he did not know, and probably did not care. At all events, they were forced to respect him, and that was his ambition.

It was in *The Lancet* office where it still stands, in the Strand,—in 1851 probably,—that I first saw the founder of *The Lancet*. A small room on the second floor was his favourite study. Permission granted me to look at the room a few days ago showed it much as

it was then. The old editorial desk was still there. The chair by the window, from which the editor, in moments of relaxation, would play with masterly skill his favourite game at chess, dictating the moves without looking at the board, and winning many games, while he was at the same time sketching out a leader, or putting together the details of an inquest still adjourned, was still there. The couch on which, as he more than once told me, he could lie down and fall asleep in five minutes at any time, was there. Everything seemed to be so precisely as he left it, I experienced a kind of sudden disappointment that he was absent. It vividly brought back our first interview.

A very early paper of mine, read before the Medical Society of London, on the "Antiseptic Properties of Gases," had brought the first meeting about. In that paper I had described the details and the results of a research then, I believe, quite new, to ascertain if dead animal structures, pathological and other, could be preserved from decomposition in different gases and vapours. I had found that all the anæsthetic vapours, as far as we knew them, possessed some antiseptic power, and that chloroform vapour was a splendid antiseptic. I had found that ammonia possessed the same quality in a marked degree, but that the gases of which it is composed were less marked in this respect when they were used in their separate form. I had ascertained that when arsenic was combined with hydrogen, as arseniuretted hydrogen, a gaseous product was at hand which prevented ordinary decomposition absolutely, but which led to a transformation of muscular fibre, resembling, in the closest degree, fatty degeneration of the same kind as was then being much studied in morbid specimens of fatty degeneration

of the heart. These, with many other facts, had made up my paper, and it, together with the structures by which it was illustrated, two or three of which still exist, attracted the favourable comment of one of the members of the staff of *The Lancet*, Mr. J. Fernandez Clarke. That *The Lancet* should speak well of so young an effort was interpreted by Dr. Snow and other friends as a very good sign, and soon afterwards I had an intimation that Mr. Wakley wished to see me. In this manner I made his acquaintance and his friendship.

The plate published on the opposite page is taken from a bust of Mr. Wakley, and is an admirable likeness of him at the period of which I speak, in so far as the face is concerned. But the whole man had to be seen before any judgment could be made of bearing and style. Then the power of the man was apparent ere ever a word was spoken. There was a massiveness which meant strength, and a physical proportion with a balance of power which impressed every one, and which suited the office of coroner so well as to be the subject of general comment. I heard it once said of him, in his court, that he was every bit a judge without the wig and gown; and no statement was ever more truthful. When standing he was sufficiently tall to be commanding,—five feet ten to eleven, I should say, was his height,—and of good comely stature. When sitting he looked taller than most men, and at the dinner-table sat somewhat above the majority. He was altogether of large build, with a chest of forty-five inches girth at least, a body well shaped, limbs strong, hands rather large, and grip of them unmistakable when he shook hands, which he was rather chary of doing unless he took a sudden liking, or by acquaintance some time con-



Thomas Wakley





tinued, learned to like a stranger. His face was large, and intelligently and flexibly, though firmly moulded; features well developed; forehead high rather than broad; eyes small, grey, piercing, and seeming to lie far back, from a habit he had of making his eyebrows tent over when he was earnestly looking at any person or object. Complexion fair, with slight russet ruddiness, and in the flaxen curling hair just a tinge of the same sort. The mouth well set, the lips easily and quickly compressed during emotion, so that, though of moderate size naturally, they then looked thin. Ears well shaped, and lobes large, "showing," as Laycock said, "the truth, sir, of my theory—large ear lobe, active brain." The voice round and commanding, but sometimes rather spoiled by a sudden sharpness or thrill almost tremulous, and at first peculiar to a stranger listening to it. Mode of speech slow, collected, firm; no possible mistake whatever about what was meant, whether for exposition, for praise, or for blame. No Lord Chief Justice could sum up more clearly or incisively than this coroner for Middlesex.

I give here as they come back to me the general impressions which I took of Mr. Wakley at my first introduction and afterwards. There were, however, other characteristics, which I, and all who understood him, soon discovered. Behind what seemed a rugged and determined front, there was a gentleness of disposition which passed even to tenderness, and surprised those who merely knew the man by what was said of him. If in him fire of passion ever glowed, he concealed the fire better than anyone I ever have known. The members of his staff might be disturbed and irritable, he never. "What do you do when you are angry, Mr. Wakley?" I one day asked him. He looked at

me with a twinkle of his deep-set eyes, and, with an expression that carried a suppressed laugh, answered, "I sleep it off, my young friend, as you must learn to do." In parenthesis, I may say that I took the lesson to heart until I became master of it, and a very useful lesson it has been to me.

John Fernandez Clarke told me that he had never seen his "chief" angry in the true sense of the word, and that when he, the "chief," saw those about him inclined to anger, he "smoothed them down in no time." But he was not, because of that, the man to put up with bad temper or wilful annoyance. On the contrary, he had the most masterful control over what was objectionable, using, when it suited him, a rich humour of criticism which answered admirably. I recollect one good instance in point.

A rather clever man, who had gone through a university education, had been afterwards to Germany to study organic chemistry under one of the great masters there, and who had come to London to start as physician, *summum bonum*, tendered to *The Lancet* a course of papers of an advanced type. The offer was accepted, perhaps, for editorial after-peace, a little too readily. The young author had two possessions, a superb idea of his own greatness, and a name amongst the most commonplace of English surnames. From both these causes he was in danger of being any day completely swamped in Babylonish banter. He was saved swamping from the first cause with great difficulty, because he had no originality of conception to sustain the learning which floated him. He was saved swamping from the second cause more surely, because his parents, with fine forethought, had, in baptism, added a prefix to his surname which distinguished it admirably from the vast multi-

tudes of the same patronymic. So he managed to float pretty safely, and even to cut a fair figure in a smooth sea. There was such a sea on when his papers appeared in *The Lancet*, and he floated so gaily that he thought he had become the mainstay of the ship that carried him. Thereupon my gentleman grew bounceable, and worried the sub-editor so terribly that he, poor man, went nearly wild. He couldn't get copy without infinite trouble; he got copy, had it set up, and received it back so "dirty," it was thought better to have it all set up anew than to revise proof. Then, if in the final printing there should be a technical error, the row was fearful. This petty tyranny reached at last such a pitch, that the "sub" went to the "chief" to explain his grievances. The "chief" counselled a try at further forbearance, since the papers answered, and the trial would soon be over. The experiment of further forbearance was made, and failed. Matters got worse, and, finally, so much worse that it became difficult to get the journal out at the usual time of publication. To his "chief," therefore, the sub-editor once more resorted. "What am I to do, sir?—I can forbear no longer." "Present my compliments to Dr. —, and tell him that from henceforth you have my instructions to announce his papers as usual, but that we intend to drop his——" the baptismal part of his name. The remedy was droll as it was drastic, but it answered perfectly. The sub-editor had no more trouble.

The humorous side of Mr. Wakley's character occasionally came out in another form, in the way of shrewd remark on some word or topic. One day the card of a visitor was brought to him, bearing a name he did not know how to pronounce. The name became familiar in time, but at that period was not so. When

the visitor had chatted awhile, Mr. Wakley, getting interested in him, asked him upon leaving, how precisely to pronounce so singular a name, and having got the information, added a word of advice. "You promise well, sir, very well. You have ability, and, I believe, perseverance; but you must be very, very cautious because of your name. Yours is a name that will require the most delicate manipulation."

These playful sallies were frequent, and relieved his utterances from dead Saxon weight and oppressiveness. Saxon, mainly, by race, Saxon by name, probably from one of the towns Wakerly, after Saxon fashion of naming, there was just a touch, both in physique and mind, of Keltic stock too, which gave point to weight, and brightness to solidity. Sir John Forbes, who did not like him over much, said he was a Nasmyth's hammer; he could smooth a curl or crack a pate with equal facility.

The perfect control over passion which Mr. Wakley possessed, combined with the force of character and intense vitality with which he was endowed, was the secret of his remarkable success under circumstances that would have killed men equally powerful but less restrained. Through affection the will of a child might influence him, but he bowed his will to no domination, and knew no master. He came from Membury in Devonshire, to carve his way, in London, as a surgeon, with as little help as any man ever had. He might, with his pre-eminent ability, have gained any place in the old grooves, if he would have bent to the official yoke. He would not bend an inch. He was thus, from the first, considered fractious and over-independent, subjecting himself, thereby, to the strangest accusations, and exposing himself to legal and even



personal risks which in these days are unknown. His self-control was his safeguard in all these trials of his courage and endurance, or I had better said his self-control and self-reliance, for in all his legal contests, amounting to about a score, he, from first to last, acted as his own advocate, and except on two occasions, or three at most, came off victor. It is but just to him to add that when a struggle was over he was always as ready to forget the past as he was to buckle on his armour again for another affray. His enemies said that he enjoyed combat. I think that is doubtful. But he was a born reformer of great abuses, and he knew that it was his fate to be belaboured freely and belied roundly, as all such men are, by the respectabilities as well as the rowdies of society.

Why he should at first have taken up the subject of reforms in the profession of medicine he never knew himself, except that the work had to be done and somebody was bound to do it. These were his own explanations in almost the words I have written, as I remember to have heard them. He saw the most flagrant abuses and monopolies all through his student's career; abuses in hospitals, in colleges, everywhere. To remove these, heroic of heroic treatment must be adopted. The heroic of heroic treatment for physical disease was then practised by the use of the tiny instrument of steel which, according to Byron, killed more than the lance, and which, by the way, gave the *coup de grâce* to Byron himself. The body medical, by analogy, required the same treatment. Hence the origin of *The Lancet*.

*The Lancet*, the present leading medical journal of the world, for all public views of it, was the inspiration almost of a minute. The intention of the journal and the name of it came, its author told me, into his mind

at the same moment, and the first number which came out, on October 5th, 1823, was so little premeditated that it appeared before the material for the second number had been thought about, except for a lecture by the great Sir Astley Cooper. That first number is of thirty-six pages, double column, and octavo size. A seedling indeed. The first volume for three months contains less matter than two weekly numbers contain now. To show the entire independency of thought with which it was launched, the first number of *The Lancet* was published on Sunday, and continued to be so published for a long time afterwards.

It was a little *Lancet* as it began, but any one who will read it, even now, will be interested in it. It was audacious beyond measure. It presumed to connect the art and mystery of medicine with the great world of which that art and mystery was a concealed part. It said to the public, Read physic. It said to the profession, You and the public are one and the same. It bade the professors of physic study in its own pages, not medical subjects only, but everything else that was of public interest. Chess, the stage, the concerts, odds and ends of stray stories and anecdotes, some exceedingly good, these filled the first *Lancets*, one of Sir Astley Cooper's lectures, models still of professional art as well as of science, having the place of honour.

The rectification of abuses, the reformation of medicine, through good report and through bad, was the one grand aim of *The Lancet*, everything in it a means to that end. That it would ever become the widely spread, the widely read, the influential, and, I ought to add, the profitable journal it has become, never entered into the early hopes of its founder. The last thing he dreamed of was that his keen *Lancet* would

become a rich fortune for his children. He used it with a strong hand to correct what he considered abuses; and his exemplary wife—for he married early in life—not wishing him to pursue the regular professional prescribing course of medicine, he left that line of practice entirely, to make journalistic literature and public work his sole occupation.

As a matter of course, troubles sufficient soon came upon the enterprising, merciless, uncompromising editor. Scope for any amount of criticism lay before him, and he availed himself of the opportunity to his fill of it. In the first years of his work the sick hospitals of the metropolis were the closest boroughs in the country. Men got appointments in them who had neither the eyes to see, the ears to hear, nor the tongues to teach. One man alone in one of these “mummeries” might rule the whole. Mr. Wakley went tilt at this scandal. The officials might deliver lectures within those sacred walls, and students might listen after they had paid for the privilege, but nobody might poach there. The game laws of physic were exacting, and firmly kept. The editor of *The Lancet* did not fear. For good or for bad those lectures shall come before the public eye. If they be good, if they tend to the mitigation of one pang of human suffering, the whole profession, and through the profession the whole public, shall have the benefit of them. If they be bad, nay very bad, the whole profession, and through the voice of the profession the whole public, shall have the condemnation of them. The contest for freedom against monopoly soon began. In the third year of *The Lancet*, Mr. Abernethy, an able, unflinching Scotch surgeon, whose name has become a sort of password for a peculiar kind of wit of the quiddit type, and of whom I shall, if I

live, have something to write at a future day,—Mr. Abernethy applied for an injunction that his lectures should not be published. The fight was a tough one, and a whole number of *The Lancet* seems to have been devoted exclusively to the details of that action. In various minor ways the same opposition was renewed, and renewed again. In the end, the press was victor, and although loaded with all sorts of calumnies, the founder of the free press of medicine stood master of the situation. The dissolution of the Abernethian injunction, realized in the early part of 1826, set the medical press at liberty.

An action of a more personal character, an action for libel taken out by Mr. Wakley against Dr. James Johnson, a rival editor of a distinguished and classical journal devoted to medicine, with two other similar actions against men less known in professional circles, three trials in one year, seemed only to whet the courage of our indomitable editor. For, so soon, or hardly as soon, as these were settled, he opened fire on the great medical and surgical corporations. The Royal College of Surgeons, snug, rich, and lofty, came first under his lash, the effort leading, indirectly, to two more actions for libel, but ending on the whole triumphantly for him.

A firm footing gained in the matter of reform in the colleges and in the universities of Scotland, as a result of the castigations applied to both, a general outline of reform for medicine altogether was his next idea, an idea which up to this day has never been completed. What the basis of that great scheme of reform was, which Mr. Wakley first suggested, and which he continued to defend to the close of his life, is worth a moment's attention.

By various legal provisions the government of the medical profession in these Islands had become deputed to a few central colleges or corporations, such as the Royal Colleges of Physicians and Surgeons, and the Apothecaries' Companies. These, like so many medical Vaticans, dispensed their privileges on their own terms. Sometimes, when they were sedulously looked after, they assumed an air of virtue, and exacted some proofs of learning from the candidates for their favours. Sometimes, when they were not looked after, they sold their indulgences in a rather loose and profuse manner. Whether virtuous or not in the way of granting licences, they were very domineering and resolute in defence of their own existences, privileges, and dignities. Mr. Wakley, a man of the people, fighting always for the people, and through the people, did not see the justice or the usefulness of this piecemeal, self-meal arrangement. He was of opinion that, as far as was possible, medicine should be a free science and a free art, as chemistry and astronomy and mathematics are free. But, inasmuch as medicine deals with the lives of the human race, and as, therefore, for human safety, those who practise medicine should be obliged to prove their competency, he was for legal restriction of the practitioner. To that extent he was in accord with his contemporaries. He differed from them only when the mode of carrying out the necessary protection came under discussion, and then he differed absolutely.

The protection, he said, which the public requires must be fully granted, but it must not be a capricious protection, and of all things it must not be embarrassed with organic professional differences, so as to be a bar to independent, individual, professional freedom. All men



entering medicine, at their start, should stand equal. Let them, before or afterwards, win any voluntary distinctions they can, and rise before their fellow-men to any position. Genius will carry itself, and may be left to fate and industry. But the gate of medicine, let that stand independently. Admit no man by a Royal College of Physicians more than by a Royal Society. Admit by one legal entrance, and let the broadest and most public power, the most important and the least self-interested power, keep the portal under guard. When a man well accredited comes there for admission, ask him no questions as to where he has studied, or who have been his pastors and masters; but, without prejudice, or patronage, see if he can pass through that portal; if he can, let him, and leave him to answer afterwards to and for the public according to his work and ability. Moreover, that the ordeal of passing the portal may be conducted with a fairness admitting of no rivalry and no question, let it be carried out by the State. Let the State, if it like, take into its confidence eminent representatives of medicine; let it give to the profession of medicine, altogether, a voice of representation, as the servant of the State; but let the State hold the power, and accept the responsibility. Then medicine will truly be amenable to law, and her practitioners will be legally protected by, and obedient to, a code of the simplest kind, which everyone can read with understanding. Then, too, medicine will be great, because she will be an integral part of the people.

As briefly as possible I have here sketched out the plan of medical reform which the founder of *The Lancet* laid down as the basis of reform. How any public legislator in or out of the profession of medicine, how

any public or private man in the profession, who is or is not deriving advantages from corporate rights, if he be clear sighted, can hesitate to accept this basis as the one and only one upon which medical science and art can ultimately rest, is difficult indeed to comprehend. Yet this basis, so simple, so natural, remains unbuilt on, notwithstanding that those who are most opposed to it must be sure, in their own minds, that there will be no peace until it is the acknowledged and utilised groundwork of medical organisation.

As the new journal gathered strength new objects were introduced into it. The formation of a medical benevolent fund was broached in persuasive terms, and soon a battle commenced on behalf of the principle that the ancient office of coroner, once an office held only by valiant knight or man of arms, then by any man of mark in a town or county, then by members of the legal craft, should only be held by one who could scientifically inquire into the causes of death, that is to say, by some practitioner of the medical profession. This battle was fought first in *The Lancet*; then at the polling booth by the electors, the freeholders of Middlesex; and the battle, after a preliminary defeat, was won ultimately with flying colours. The editor of *The Lancet* became the coroner for Middlesex. How excellently he fulfilled the duties of that important office I have already hinted, but I have said nothing that at all conveys what his work in that office amounted to. The labour of it, now distributed to three coroners, scarcely interfered with his other labours. He never seemed to hesitate or pause or tire. He never lost dignity. It mattered not where he held his inquisition,—in a public house, in a private room, in a board room,—he never lost dignity. There was no one dared trifle with Mr. Wakley or wrangle

with him. The members of his own profession were specially on their guard ; there was that keen observation upon them in evidence which they could not ignore ; and, there was that awful *Lancet* in reserve which they could not forget.

The coroner was still upon occasion the surgeon. Once on entering an inn, in High Street, Marylebone, to hold an inquest there, a woman who had fallen down was bleeding to death from a rupture of a large varicose vein in the leg. The coroner, finding that no other medical man was present, proceeded at once to put on a compress and arrest the flow. The operation over, he told the jury that had to sit before him on the case to which they had been summoned, that three times since he had been coroner he had held inquests on persons who had died from loss of blood, and who might have been saved, as he had saved the bleeding woman, if any one present had known the simple art of compressing a blood-vessel in the proper way. He gave them, in addition, the correct information as to the proper way, and explained how they might know the difference between bleeding from an artery and bleeding from a vein.

The election to the office of coroner led, chiefly, a few years later, to his election to Parliament as member for Finsbury, in the year 1837. He continued in Parliament until a few months before his death, and in the office of coroner until his death in 1862, never neglecting in either position any important duty. Meanwhile, as editor of *The Lancet*, he held on his warlike course, "constantly in hot water, and usually coming out cool." The Council of the College of Surgeons once threatened him with criminal proceedings ; they thought better of it and withdrew the action. The

authorities who had charge of the lunatics of the United Kingdom nearly went insane themselves, in their anger at his trenchant, reforming, exposing hand. Dr. Elliotson, bitten by the mesmeric and clairvoyant mania, crossed swords with him on that topic, to receive, despite the protecting friendship of Charles Dickens, a series of blows which brought him, once a king amongst physicians, to a state of disfavour from which he never recovered.

In the midst of so much controversy and untiring endeavour, many times wrong in particular efforts, while right in general intention, Mr. Wakley had the foresight to predict the ultimate triumph of preventive over curative medicine, and to give a helping hand, right willingly, to the preventive system in its early developments. No one more fully understood and appreciated Mr. Edwin Chadwick's remarkable report on the sanitary condition of the labouring classes, published in 1842; and although the critical Editor was inclined to be rough upon the great Sanitarian, it was, as when Greek met Greek, in spirit of rivalry rather than anger. Indeed, in all subjects relating to the poor and helpless, no champion who ever rose for them was more chivalrous than the founder of *The Lancet*. The people were his care. He inquired into the condition of their homes; he sent his commissioners into the workhouses; he supported the parochial medical officers in all that was fair, laudable, and humane.

The last and one of the boldest acts of Mr. Wakley may be said to have crowned his strong and remarkable career. I refer to his organisation of a commission of inquiry into the subject of adulteration of the foods and drinks of the people. We all know that the details of that great inquiry were carried out by Dr. Arthur Hill

Hassall, and no one but the most thoughtless, the most ungracious, the most narrow-minded, can fail to recognise the patient skill, energy, and perseverance with which Dr. Hassall carried out his task. Mr. Wakley was amongst the loudest in commendation of this excellent analytical worker. But while we are bound to render to Dr. Hassall his fullest due, and to regret how speedily his good work has, in some quarters, been forgotten, we have to remember the strong hand that supported the labour, the mind that directed it, the will that willed that it should, in spite of every obstacle, be carried out to the letter. Action for libel followed action; but the exposures were too complete. The adulterators, like the craven rogues they were, winced and threatened, but feared to face the light of day before the public gaze. One knows not which to admire most, the merciless accuracy of the analyst, or the unflinching courage and reliance of the editor.

I have proceeded too far at this present time to find room for a number of other points bearing on the work of this medical editor. The profession of medicine, all round, may be grateful to the founder of *The Lancet*. Nay, the very men whose order he most opposed may with perfect honour admit that, in respect to their order, time alone was required to show that he was right. The work of the Royal College of Physicians, which it now is carrying out, of granting licences for general practice, a work which he long ago urged them to undertake until a State examination was commenced, would have made the college the richest and most powerful of bodies. By scorning his advice the college lost two generations of licentiates, their fees, and, of far more value, their influence. If he had had his way the Royal College would, at the present moment, have



meant the whole profession in England and Wales at least, with outlying branches, if I may so say, wherever English medicine is practised.

It is, however, the great body of general practitioners of medicine who owe him most. It was for them he fought, it was their battles he won. The Medical Witnesses Act, which ensured for each practitioner some reward, if not an adequate one, for skilled service, was the fruit of his labour. For every fee which passes to every practitioner who now supplies an insurance company with a skilled report on the life of an insurer, the recipient of the fee has to thank Mr. Wakley, who not only proposed this reasonable and just demand, but, in his shrewd and business method, showed that it could be done. Through his labours the profession of medicine has already received, for its public services, from thirty to forty thousand pounds.

In the general practitioners of English medicine he had unbounded faith. A Cromwell, a Cavour, a Gladstone had not more faith in the masses and in their cause than Mr. Wakley in the masses of his profession. In his sphere he was the leader of the rank and file to their social rights, privileges, and interests; never forgetting, meanwhile, their higher obligations and nobler duties as the return for their improved position and influence in the world. It was his theory that the members of the main body of the profession are as capable as the most capable; that a general knowledge of the whole field of medicine is a grander knowledge than the most refined special; that the foundation of medicine must always be this general and applied knowledge; that the urgent wants of mankind for medical skill, outspread on all sides, can be supplied in the total by no means less universal than

by the faithful dispensation of that skill, in its total, by every practitioner ; and that as the special and commanding talent, which lights up and commands, is, at its top of brilliancy, no more than a light from the main body of the profession from which it springs and on which it depends for its support and permanency, so the main body can, if it will, rule and govern at its pleasure throughout all its sphere.

The time, as it seems to me, has come, when this main body of medicine ought to be alive to the recognition of the labours of a leader who, with industry unsurpassed, courage unsurpassed, and judgment rarely surpassed, thought such great thoughts, did such great things for them and their vocation. Never, indeed, had the profession of medicine occasion to deplore more keenly a loss by death, than that loss which it sustained in 1862, when, in his sixty-eighth year, the founder of *The Lancet* ceased to be one of the moving spirits of this little world.

I have passed, for many pages, from the personal to the general, subscribing a tribute rather than a remembrance. Let me for a moment or two return to personal recollection, and so conclude.

My introduction to Mr. Wakley led to some literary work on *The Lancet*, by which I was brought into frequent communication, always of the most friendly character, with him, until my more intimate association with Dr. Robert Willis, who had but lately resigned to Dr. Alfred Swayne Taylor the editorial chair of the old *Medical Gazette*, gave me a leaning to the *Gazette*, to which I contributed many articles. Later on the *Gazette* ceased to exist as a separate journal. Mr. John Churchill bought it up, combined it with the *Medical Times*, and appointed my friend the late Dr. Stevenson

Bushnan, editor. Thereupon I went altogether, as a free lance, on to the *Medical Times and Gazette*, and remained in that position for a period of more than twenty years.

Though for these reasons no longer associated with Mr. Wakley in literary work, and though by connection with the British Medical Association, and by helping to found the Metropolitan Counties Branch of that Association, I was really disassociated, he always remained a good friend, never failing to say a kind and encouraging word, and sometimes honouring me by sending a question which some correspondent wished to have answered, or a book for special review; while, whenever we met, I was sure to receive from him a friendly greeting, with some little exchange of repartee, which ended pleasantly. One day we met at dinner, at Wimbledon, at the beautiful home of Mr. John Churchill. Between Mr. Wakley and our host there had been a long coldness which was now made up. The guests assembled in the garden, and there I found Mr. Wakley, taking it easy, in the sun. He came up to me with a step as young as my own. "So you have gone over to the enemy, sir," he said, taking my hand. "Yes," I replied, "and you have come over." "True," he responded, laughingly, "and a very good come too, for I shall get a good dinner by it." This led us to converse on what amount of friendship there was in a dinner. I totted it up to him numerically. "A man of twenty, living on to fourscore and a bit, could eat twenty-two thousand dinners, therefore one dinner is the two-and-twenty-thousandth part of a life-long friendship." The idea amused him immensely. "I should have put that calculation," he observed, "into *The Lancet* in its early days; at pre-

sent I shall merely tell it to John Churchill, and suggest to him what a great deal he has to make up." Then he took me aside, explained a great scheme he had in view of making the Royal Free Hospital a large medical school, and, giving me an outline of part of the programme, added with dry humour, "I am about to propose, my good friend, that you and I should hold conjointly the chair of medical jurisprudence; you to do all the work, and I to get all the glory. How do you like the prospect?" The bargain was closed, but it never fructified, the general design being soon afterwards abandoned.

At that meeting the name of Marshall Hall came up, and after that the name of Sir Charles Bell. In this conversation I asked Mr. Wakley if he had ever met Alexander Walker, who had claimed the discovery of the distinctive functions of the spinal nerves. "Yes," he replied, "he was somewhat a bore, but I had a sympathy with the man, under the impression that he had a case which might have been better pleaded than it was. His book, handsomely bound in calf, was sent to me for review, but it was out of my line, and as I could find no one ready to tackle it fairly when it was talked about, it lies, as it has lain for years, on a shelf in my office. If you like to call for it, it is yours, and I hope you will make more use of it than I have." I did call for the work, and found it left out for me. I retain it still, and intend one day to give it notice in these pages, as a remarkable book long buried and long forgotten. But above all I treasure it as the last kindness bestowed on me by the greatest medical reformer of the nineteenth century.

# ON DISEASE FROM BICHROMATE OF POTASSA.

A STUDY IN INDUSTRIAL PATHOLOGY.

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**T**HE first description of the local diseases arising from the action of bichromate of potassa was given in this country by myself through a report on forensic medicine in the October number of the *British and Foreign Medico-Chirurgical Review* for the year 1863.

For the facts on which that report rested I was indebted to MM. A. Chevalier and Bécourt, who originally published them in the *Annales d'Hygiène Publique* for July, 1863.

The first observation occurred, accidentally, to one of these distinguished authors. He met with a man who was engaged in a manufactory of bichromate of potassa, and who was suffering from a peculiar ulceration of the face. The observation led to the belief that the process of manufacture was attended with serious mischief to the health of the workers.

It was some time before the full truth came to light, for Chevalier and Bécourt found a difficulty in conducting their inquiries, a difficulty which I have myself



since experienced. The masters of the manufactories did not like to tell, and the men were taught not to be communicative. At last, from the director of a manufactory at Graville, the learned inquirers obtained a series of facts, which enabled them to construct and publish one part of their report.

These first facts related to the action of the fine particles of the bichromate upon the mucous lining of the nose.

In transforming the neutral chromate of potassa by means of acid into the bichromate, the vapour arising carries with it an infinite number of pulverized particles of the bichromate, which particles diffuse through the workshop, and are easily visible in a ray of sunlight. The particles, inspired in abundance, give to the palate a bitter and very disagreeable taste; but as profuse salivation is the result, the chromate is thrown off in the saliva, and has not time to inflict any permanent injury. If, however, the respiration be made by the nose, the particles are dissolved in the layer of secretion which lies on the membrane of the septum of the nose, creating a violent pricking, a suffusion of tears, and an irresistible sneezing. In time the membrane begins to be thrown off, and portions of it are carried into the handkerchief used in blowing the nose. This process, when once started, goes on so rapidly that after a period of six or eight days the septum becomes thin, is permeated with openings, and is ultimately detached altogether. At this point, all the symptoms that have been described cease, and the workman scarcely notices the loss of the nasal partition. This process of ulceration of the septum of the nose occurs amongst all the workmen, except in those who take snuff. In these, owing to the layer of powdered tobacco which covers

the membrane, and the frequent use of the handkerchief, the evil is removed, or rather prevented.

It is worthy of remark at once, as bearing upon this part of the first report on bichromate disease, that the cartilaginous portion of structure is specially attacked. The mucous membrane of the nasal cavity lining the opposite walls is not attacked in a similar manner. It is irritated, but does not seem to undergo the process of ulceration. We shall see, in the sequel, a possible reason for this curious exemption.

On the skin,—say these authors,—on the skin in its normal state, the epidermis being intact, the bichromate exerts no baneful influence: the hand may in fact be plunged into a concentrated and hot solution of the salt, without fear; the hand may also remain covered with the salt for an entire day, without any observed effect; but if the skin is torn or abraded, however triflingly,—by the prick of a pin, for example,—a sharp pain is felt on the exposure; and if the salt be left in contact with the wound, the caustic character of the salt is brought out intensely, the cutaneous tissue is decomposed, and violent inflammation is established. These symptoms are accompanied with intense pain, especially in winter, when the cold is severe. The action of the salt does not cease until the cauterization has penetrated to the bone.

When a workman was clean and careful, he prevented these accidents; but if he were careless, and allowed the bichromate, either in powder or solution, to touch abraded parts, or sores, or wounded surfaces, he had immediately to use remedial measures, or suffer a severe penalty. In some cases, where the workmen were too lightly clad, they were attacked with violent itchings, followed by suppuration and ulceration of the moist

surface of the penis, around the glands. This condition sometimes progressed until a disorder not unlike syphilitic ulceration was presented for observation.

The effects of the bichromate were shown on inferior animals as well as man. Horses employed in the manufactory, and which walked over the salt, were attacked in the feet. The hoof would fall off, and the inflammation extend to the upper part of the leg, causing the hair to denude, even to complete denudation. Some important particulars were supplied on this point by Mr. Clouet, a manufacturer at Havre. A horse employed in carrying the bichromate was attacked in one of his hind legs. The wound became so painful as to necessitate absolute rest; the master of the horse not understanding the treatment, the suppuration went on, extending through the limbs; the skin of two legs fell off, and an enormous suppurating surface invaded the almost entire half of the body of the animal. Death occurred a month after the commencement of the malady. The writers remark that it is as though, when the decomposition had commenced, it went on indefinitely; "there is a veritable metamorphosis of the cutaneous tissues and of the flesh altogether, analogous to fermentative action."

Dogs and cats were found to be subject to similar accidents from the bichromate. These animals, from walking among the refuse of the manufactory, were frequently affected; the skin of their feet was affected, and suppuration followed. In one instance, a rat, killed in a manufactory, was found to have all his feet suppurating, and partially destroyed, as if gnawed. This peculiarity of the ulceration, like to rodent ulceration, is worthy of special remark.

M. Clouet,—to whom, as I have said, the report of

MM. Chevalier and Bécourt is much indebted,—supplied also to those authors certain facts relating to the internal exhibition of the bichromate of potassa. He stated that in a small dose, a few centigrammes, the bichromate acts as a purgative; in larger doses, say of one gramme, it acts as a poison. In the large doses, it produces colic and purging, but no vomiting. In one manufactory, certain of the workmen placed some bichromate of potassa in a barrel of cider, as a joke. The cider was rendered of a dark colour; but, notwithstanding, many of the workmen, who were not in the secret, drank of the cider. These were all affected with severe colic and diarrhœa.

We may gather from this fact, that, taken internally, the bichromate is a saline purgative. It has been considered, therapeutically, as a glandular irritant and promoter of secretion.

In a second account sent to the reporters by M. Clouet, the facts of his previous statements were confirmed, and certain further details were offered, which are of interest. Thus it was shown that both sexes are equally influenced, and at all ages; and that the affection of the septum of the nostril usually makes its appearance within a week after exposure. The disease of the septum was very easily brought about in those workmen who, having stained their fingers with the chromate, put them into the nostril. M. Clouet also observed in every case, that in those men in whom the septum was entirely removed, nasal catarrh was absolutely unknown.

It was not reasonable to let such an important piece of clinical observation as was conveyed in this report of our French confrères pass without inquiry of a practical kind. I began, therefore, soon after I had

become conversant with their labours, to institute inquiry in this country on my own account.

I carried this inquiry out first while writing the series of papers for the *Medical Times and Gazette* on "The Medical History of England"; secondly, in the collection of facts for the Cantor Lectures on Industrial Diseases delivered before the Society of Arts; and, thirdly, during investigations for the evidence rendered to the Royal Commission on Noxious Vapours. These labours brought me, for many years, into relationship with all the important industries, and ought to have brought me quickly into direct contact with cases of bichromate disease. It was not so. I was always on the look-out, but could get no facts. One friend in the north of England, who is a manufacturer of the bichromate on a large scale, and who sent me a splendid crystal, told me that in his establishment the disease was of rarest occurrence. One or two men had suffered from soreness inside the nose, but not for long; a few more had suffered when the salt came into contact with a sore place on the fingers; and one or two animals had suffered in the feet, but that was all he had ever known. The sum total was so slight he should not have thought it of any moment whatever, if I had not named it to him.

At last, about six years ago, Mr. W., a worker in a business in which bichromate is used, called on me during morning practice. He came to me on the recommendation of an operative chemist, who had read one of my Cantor Lectures, and had noticed a reference to the bichromate affection, and who thought I might be of service to a patient. The case was one in which the disease that was presented was obviously



from the action of the bichromate ; but, strangely, it differed entirely from what Chevalier and Bécourt had described, and, in fact, was a new form of disease altogether. It was a cutaneous disease, and, contrary to the opinion of the above named authors, was produced by the application to the skin of the bichromate in a watery solution containing from five to six per cent. of the salt.

The symptoms in this instance were remarkably acute and severe. The hands and forearms were the parts most affected, and, when exposed to view, appeared as if affected with acute eczema. Indeed, I regarded the affection as eczema, until I had learned the cause of it, although I was at the same time puzzled by the fact that the intense hyperæmia and weeping of surface terminated abruptly, as if by a line of demarcation. One or two other patches, which were quite dry and scaly on the palm of the hand, resembled psoriasis palmaris. The rest of the body was free from eruption of any kind, and the patient was rather amazed at my difficulty at making out the nature of his complaint, and at my pertinacity in trying to trace it to some hereditary or constitutional cause. At length he told me it was purely due to the local action of a solution of bichromate of potassa. He had seen, he reported, the same condition brought on in the same way in his fellow-workmen, some of whom had been obliged to give up the occupation.

In this case the patient had worked at the business for several months, without suffering more than an occasional rash or redness. A few weeks before his visit to me the rash increased, and continued until it was attended with severe pain and irritation, with some fever, and with such accession of burning at night as

prevented sleep. The irritation ceased as the weeping or fluid-discharge came on, and had by this time considerably abated.

As a matter of course the patient had now ceased to expose himself to the cause of the disease, and he continued to improve so steadily that in the course of two months he recovered. The most troublesome symptom in the later stages of the affection was an intolerable itching at times, attended with the casting off of a drift or bran-like exfoliation, such as is seen after pityriasis.

About, or a little before, the time that this case was presented to my observation, Mr. J. D. Colton communicated to the *British Journal of Photography* an account of his own symptoms from the same cause. His description, for a knowledge of which I am indebted to another similar sufferer, Mr. F., runs as follows :—

“It has not only been my misfortune to have suffered the greatest agony for weeks together, but I have had the opportunity of carefully noting all the effects of such poisoning upon others as well as upon my own constitution. Such extreme cases are, I grant, of rare occurrence; but as all systems are liable to the action of the poison to some extent, the subject is well worthy of attention. The effects of bichromate, when swallowed, are quite different from those of which I am writing; and, as there is an abundance of information upon the former subject in print, I will confine myself to the local poisoning simply.

“The first symptoms of local poisoning are heat, itching, and tingling upon the ends and middle joints of the fingers, which, when examined, are found to be covered with minute, irregular red patches, upon which are numerous small elevations clustered together. In





ERUPTION FROM POTASSA BICHROMATE.

the course of a day or so these enlarge to the size of a small bead, and are filled with a limpid fluid. In a short time these clusters run into each other, forming large blisters, which dry and crack open, causing the most acute pain and itching; at this stage pustules begin to form over the body, mainly down the spine, on the neck, left side, arms, and ankles, accompanied with considerable constitutional affection, such as sickness, headache, thirst, fever, sleeplessness, and loss of appetite."

The next case to which my attention was called was that of Mr. F. himself, to whom I have referred a few lines above, and who consulted me on the 29th of November, 1882. Mr. F. is employed in the autotype process of photography, in which the bichromate is used in solution of five per cent. for rendering gelatine insoluble in warm water. He has no constitutional taint nor tendency to cutaneous disease, and for a considerable time he suffered from nothing more than occasional erythema from the solution. Some months, however, before his visit to me, redness and irritation became permanent and, at times, painful on the hands and arms, to about the middle of each forearm. The appearance in this case was not of eczema, but was so singularly true to *pityriasis rubra* that I should have entered it in my note-book as a good example of that disease in its subacute stage. At the same time, there was in the palmar region an appearance very like psoriasis. Mr. F. was so good as to have photographic views taken of the affected parts, which have been very faithfully coloured, and are given on the accompanying plate.

The history of another case is that of a young man, twenty-four years old, Mr. C., who suffered about six



months after he began to work with the bichromate. He was engaged in granulating, that is, dissolving the salt in boiling water, and then allowing the solution to cool, stirring it all the time, in order that the salt may not crystallize. In this way he was exposed to the splash of the granules on his arms, the result being an eruption on both arms, extending from the wrists to the upper part of the forearm, and accompanied by redness, heat, and such great irritation when in bed, that he could not sleep for it. This eruption lasted for six months, but on resting from the work for a few weeks it passed away, with a free exfoliation of bran-like scales. Later, on re-exposure, he has suffered from a few circular patches on the front part of the forearm. In these patches there was redness, with bright elevations and considerable irritation.

In the first attack, in this instance, the eruption was at once so general that no distinct circular patches could be detected; but in minor attacks the eruption has usually assumed the circular form, the patch varying in size from a shilling to a florin.

In another case, that of Mr. G., who works in a weak solution of the bichromate, there was constant recurrence of the cutaneous eruption, but always in a mild form. The eruption was in patches on the hands or arms, was red, with raised points at first, and was attended by intense itching, which was increased, invariably, by warmth. If this patient left the work for a few days recovery followed in regular course. The general health was good, but during one attack there was sympathetic swelling in the axillary glands.

In a fourth instance, Mr. D., aged thirty-six years, suffered at the end of six months, after being exposed

to a weak solution of the bichromate used in the auto-type business. In his case the eruption continued for eighteen months. It came on first as a red rash, with raised points and chapped lines, and with intense irritation, much increased by warmth. In a little time there was weeping from the inflamed surface, and the surface became covered with open cracks, which were very painful. On leaving off the occupation the disease passed away without any treatment whatever.

In a last case (that of Mr. L., who had worked with a weak solution of the salt for fourteen years) there had been no eruption at all, but twice a deep and troublesome ulcer on the hand from the contact of the solution with an abraded surface. The ulcers did not heal for many weeks, and have left deep depressions. Mr. L. also suffered once, very severely, from inhaling the dust of the bichromate. He had intolerable irritation, uncontrollable sneezing, and profuse nasal discharge. The symptoms, fortunately, subsided after a few hours' duration.

#### THE QUESTION OF TREATMENT.

I have referred, incidentally, to the question of treatment in these cases, and would now dwell shortly, but specially, on that subject. Chevalier and Bécourt state in respect to the cases they witnessed, that, as a preventive of the nasal disease, the use of snuff seemed very effectual. In other cases the workmen applied a wet sponge before the nostrils when they were exposed to the particles; and that plan also appeared to be advantageous. When the skin was abraded, and the chromate had produced ulceration, it was the best treatment to wash thoroughly with a feeble alkaline water; then, if inflammatory action followed, to poultice,

and afterwards to apply freely sub-acetate of lead in solution.

Mr. Colton, from his observation, thinks that the treatment—owing perhaps to the rare occurrence of these cases—does not appear to be well understood. He sought medical advice, but obtained no better remedy than that of allowing nature to take her course, and aiding her by paying strict attention to diet and habits. The following treatment he recommends as successful :—

Abstain from drinking beer, spirits, or coffee, and eat no potatoes, brown meats, salt fish, or sugar. Take plenty of open air exercise and use no tobacco. Take one drachm of liquor potassa in a cupful of barley water thrice daily, before meals, for five or six days; after which take three times daily of tincture of serpentary (American snake root) one drachm, with five drops of laudanum. The parts affected must be bathed frequently with dilute solution of sub-acetate of lead. The foregoing treatment is to be continued until the sickness and pain have subsided, and the patches have lost the fluid they contained and begin to heal. The hands and arms should then be treated with resin ointment, and a mixture should be taken of ammonio citrate of iron, with iodide of potassium and bromide of ammonium, made into solution with camphor water.

It is generally believed, he says, that washing the hands in ammonia after using bichromate prevents the poison from affecting the surface by neutralising free acid; but this, he adds, is quite a mistake; a solution of bichromate rendered perfectly alkaline with ammonia poisons quicker than an acid or neutral solution.

In the cases I have seen I very much doubt whether any treatment was of much avail. It seemed to me

that recovery commenced, when the cause of the evil was withdrawn, and nature followed her own course.

I see quite well how recovery would follow on removal of the local cause without any other aid, and I do not see any rationale for the remedies that were tried, with one exception. It is clear that the local application of the sub-acetate of lead is attended with good results in relieving the local irritation, and perhaps in arresting the discharge. I have used it with certain benefit in the cases under my care.

In the course of time this industrial disease will, I doubt not, become a mere memory of the past, like the phosphorus necrosis, of which now, under an improved hygiene, we hear so little. We have only to invent a plan by which the bichromate of potassa shall be prevented coming into contact with any of the surfaces of the body, to stop all the forms of bichromate disease. The wearing of a mask while at work or while exposed to the bichromate dust, would prevent the nasal affection, and the wearing of an impermeable glove or gauntlet while working with the solution would prevent the cutaneous affection.

#### A PATHOLOGICAL COMMENTARY.

Beyond the question of treatment there is much that is of moment in these curious phenomena of disease from potassa bichromate. As accidental syntheses of disease, they are, in regard both to etiological and pathological studies, of singular value.

Let us review the facts. Here is a substance, from the laboratory, of a purely inorganic character, which substance applied to the surfaces of the living body produces certain local diseases, closely similar, if not identical, with certain natural diseases, the causes of which are extremely obscure.

The bichromate in one class of cases sets up a special ulceration of cartilage, which, once commenced, continues until the cartilage is destroyed. Again, introduced into the cellular tissue, it sets up a form of ulceration, which, in some instances, assumes the character of progressive rodent ulceration. Once more, applied to the skin it sets up the symptoms which particularly characterize the time-acknowledged diseases of allied type, eczema, psoriasis, and pityriasis.

We gather from the observation of these phenomena that a certain agent, acting locally in the purest manner, leads by its own local action to specific local manifestations. The action is so purely physical, it can, in this case, be literally observed as if it were an experiment carried out in the laboratory. There is induced an intense hyperæmia from the local action, as if there were an intense obstruction to the passage of blood with congestion, as the result. There is passive exudation, death of structure, and exfoliation.

To understand the modus, it may be well to look at the action of the bichromate on gelatinous tissues out of the body.

When gelatine has been subjected to bichromate, it is rendered insoluble in warm water, after it has been exposed to light. The autotype process is based upon this principle, and is carried out as follows:—Paper is coated with gelatine containing pigment, and is then made sensitive to light by immersion in a solution of the bichromate. Finally the paper is dried, and is ready for use. To produce a picture on the paper, the sensitive gelatinised pigment paper is exposed to light behind a photographic negative, for a sufficient time to print it. After exposure the print is placed in warm water (90° to 110° F.), when the gelatine in those parts



which have been protected from light dissolves away, leaving the other parts, where the light has acted, and which forms the picture, undissolved, with the pigment imprisoned in it.

By a process similar to this, or identical with it, the bichromate, I believe, acts upon the gelatinous structures of the body. It changes their molecular condition, and renders them insoluble. That leads to an arrest in the living dialysis and in the transudation of fluid. The arrest leads, in turn, to perversion of function, to congestion, and to all the subsequent phenomena.

Here then is a suggestion for a new departure in research. Are there no other substances capable of producing similar local changes? Is it not very possible that on the surfaces of the body some oxidized products, locally evolved, may lead to similar molecular changes on gelatinous structures, and to similar local manifestations. I think it is most probable, and that in the study of the action of light on local diseases we touch, etiologically and pathologically, some great discovery.

One observation more commands attention. Mr. Clouet, who was, obviously, a clear-headed observer, recording merely what he had seen, tells us that in every case where his workmen lost the septum of the nose, there was permanent future exemption from nasal catarrh. I think it deserves to be studied whether this is not a clue to the origin of another very general, though mild, affection. May it be that the impression on the nervous expanse, which sets up nasal catarrh, is always on the septum, and that the other phenomena are reflex from that primary effect?

## OPUSCULA PRACTICA :

*USEFUL NOTES FOR BUSY PRACTITIONERS.*

“There are mites in science as well as in charity.”

BENJAMIN RUSH.

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### *SPHYGMOGRAPHIC ANEURISMAL TRACINGS AS DIAGNOSTIC SIGNS.*

**I**N cases of doubtful aneurism of the great arteries in the cavities of the body, the sphygmograph comes into use with excellent effect. In order to apply it for this purpose, first take a reading from the radial pulse and fix that for comparison. Then place the sphygmograph firmly over the suspected pulsating part and take a reading there. Next compare the tracings. If the tracings correspond in indicating arterial stroke, and if there ought, naturally, to be no detectable pulsation at the suspected pulsating part, the evidence may be considered all but absolute that there is an aneurismal dilatation. If the pulsation were induced by a healthy artery pulsating beneath a tumour, or beneath an organ of the body,—as the pancreas, liver, or stomach,—the natural arterial tracing would not be educed; there would be nothing more than irregular up-and-down strokes.

In comparing the readings of the radial pulsations with those of an aneurism, the character rather than the largeness of the outlines must be taken into account. The outlines may correspond, and their sizes may correspond, and when this occurs the evidence is the more striking. But in matter of practice size is not of much moment. The outline is the thing to observe, and a distinctive difference may in some cases confirm diagnosis. For example. I was consulted about a doubtful case of aneurism of the aorta, at the arch or at the innominate. The reading at the right radial pulse was very feeble but distinctive. By careful manipulation I obtained a perfectly corresponding pulse reading in the chest, at a point three inches above the right nipple in a line drawn from that nipple to the origin of the left sterno-cleido-mastoideus. But my pulse reading here was much larger than the reading at the radial. The systolic line was longer, the angle was more acute, the diastolic or descending line was longer, the aortic cleft more defined. The inference I drew from these observations was that there was an aneurismal dilatation in the ascending aorta, extending to the innominate and interfering with the force of the arterial stroke through the right subclavian and its branches, a diagnosis which has proved to be correct.

The grand point, however, which this mode of diagnosis settles is that it differentiates between an actual aneurism and an artery pulsating on a tumour. In the days of Harvey this point of diagnosis was once under practical discussion between Harvey himself and one of his cotemporaries, and it has remained a difficulty up to the present time. I believe the difficulty is now well nigh solved by the sphygmograph. Wherever there is a direct arterial pulsation a sphygmographic tracing

can be obtained. Whenever the tracing gives the impulse stroke, the recoil, and the notch, an artery is directly writing the message.

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*DISCOVERY OF MORPHINE IN THE URINE OF A  
MORPHINE HABITUÉ.*

INCE the publication of my paper on Morphia Habitués and their treatment in the January number of the ASCLEPIAD, nine new cases have come before me for study, each one of which has presented some novel practical feature. I refer, now, to one fact only, and that because it enables me to offer a new observation, which has not previously been proved, bearing upon a mode of elimination of the poison.

The Habitué to whose case I draw attention was a man in his forty-seventh year, tall, plethoric, and ruddy. He commenced to take morphine by the mouth, for the relief of pain, in the year 1874, his first doses being two-thirds of a grain of the acetate. Continuing the use of the drug in steadily increasing doses, he became habituated, at last, to no less than twelve to thirteen grains of it three times a day; that is to say, to from thirty-six to thirty-nine grains of morphine acetate every twenty-four hours.

He visited me for the first time on July 1st, 1884. He was then taking thirty-eight grains daily, and in the preceding seven days had swallowed over half an ounce.

I will supply the details of the symptoms and peculiarities of this case in another and more elaborate essay. My object at this moment is to explain that,

owing to the enormous dose of the morphia daily received by this patient, I thought it would be worth the trouble to ascertain if any of the alkaloid was being excreted by the kidneys. I therefore obtained twelve ounces of fresh urine from the patient on the 2nd of July, a day on which he had taken thirty-six grains, and sent it direct to the laboratory of Mr. A. Wynter-Blyth for analysis. Mr. Blyth was good enough to make the determination for me at once, and reported that the urine "yielded morphine equivalent to 1.2 grain of morphine acetate." The morphine was satisfactorily identified, turned into the crystalline hydriodate, and gave the usual reaction with iodic acid.

On the 3rd of July the quantity of morphine acetate taken by the patient was reduced to twenty-one grains. The same quantity of urine yielded the equivalent of one grain of the salt.

On the 13th of July the quantity of morphine acetate taken by the patient was reduced to six and a half grains. The same quantity of urine yielded 0.18 grain of the salt.

Voit, in the case of an opium eater, failed to detect morphine in the urine, but did detect it in the excretion from the bowels. We have evidence, therefore, now, that when morphine is taken into the body in large quantities, it may be eliminated by two channels, by the bowels and by the kidneys. This tallies with what I observed in pigeons subjected to enormous doses of opium, the compound excrete from them having yielded qualitative evidence of the elimination of a small but undetermined quantitative portion of the alkaloid.

While the fact of some direct elimination is thus proved, it must at the same time be admitted that the amount discharged is excessively small in comparison



with that taken into the body. How the larger part is disposed of in the organism, into what it is resolved there, and by what secondary means it is removed, are questions which at present remain altogether unanswered.

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*USE OF THE FARADIC CURRENT FOR THE TREATMENT  
OF STRABISMUS.*

 YOUNG gentleman suffering from internal strabismus of both eyes, was brought to me bearing the following history. He was eight years old, had been born free of strabismus, and had had no opportunity of acquiring the deformity by imitation. He had suffered about twelve months previously to my seeing him, with dyspeptic symptoms, and was believed, by a medical friend, to be affected with round worms, for which he was treated. The diagnosis was not confirmed by the results of treatment, and the strabismus remained unchanged. At the time of his first visit to me, the patient was in good spirits and well nourished; but he was rather anæmic, and occasionally suffered from constipation of his bowels. The strabismus was very extreme, and the question occurred to me whether the operation of division of the internal rectus, on both sides, should not be performed.

As the parents were averse to operation if any other measure could be employed, I determined to try if the external rectus could be brought into contraction by means of a gentle Faradic current. To carry out this intention I used a Stohrer's battery. I made the poles terminate in two silver probes, insulated to within the eighth of an inch of their terminal points or bulbs. These terminal points I covered neatly with moist

cotton wool. Setting up a just perceptible current, which I could feel by testing on my lower lip, I brought the terminals to bear on the eyeball at the outer canthus of each eye, pressing a little inwards, but not sufficiently to give pain. An assistant then very gradually increased the current until it told on the external recti muscles, causing both to contract, and to bring the eyes nearly straight by overcoming the antagonizing muscles. The effect was so decisive I persisted with it daily from May 16th to June 26th. At the end of which time the strabismus seemed to have been completely removed.

In September, the treatment having ceased for nine weeks, the patient was brought to me again, the strabismus having gradually returned, to a slight extent, but most remarkably on the right side. I there-upon repeated the galvanism for ten days, and again with good result. On the January following, I once more saw the patient, and found the left eye quite perfect but the right a little inclined to the inner side when he was fatigued. The deformity was, however, so slight it was allowed to remain, being rarely perceptible.

I note, upon this case, that it supplies an all but absolute cure of strabismus, through excitation of the muscles opposing those which drew the globe out of the natural position. The question then comes, how far this treatment may be extended in cases of strabismus, with the chance of success. Imprimis, the method is one which might, with care in the administration, be tried in every case without injury, and it may be that it is applicable to a large range of cases.

I should suggest that the treatment should be specially tried in cases of acquired or induced stra-

bismus. It can hardly be expected to be successful when there is actual shortening of muscle. But there is a class of case in which the defect of position is due to relative loss of balance between opposing muscles, one muscle being partially paralyzed, and so rendered, relatively, inactive. These cases yield the worst results of operation by the knife, and if they can be met by the plan of treatment with the Faradic current, an advance will have been secured.

In adopting this treatment, the practitioner must expect no result from one or two applications. He must pursue the plan for several weeks, systematically and daily; and he must be so gentle in his manipulation that no injury to the globe of the eye shall be possible. Most importantly he must be careful to keep the points of his conductor continuously moist while the current is being passed.

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#### *A SIMPLE DISINFECTING LAMP.*



READY means of employing sulphurous acid gas for disinfection is to produce the gas by the process of burning bisulphide of carbon in a lamp, after the manner of a good-sized oil or spirit lamp.

I take an earthenware or metal lamp, fit it up with the cotton wick in the usual way, and then charge it with a mixture of equal parts of common benzoline and of carbon bisulphide. The lamp so formed is lighted in the usual way, and gives off when burning free quantities of sulphurous acid. The lamp can be moved about from one part of a room to another, into cupboards and closets, and into wardrobes and drawers

and boxes, with so much facility that it becomes a most useful means for disinfection.

The lamp can be further employed with advantage for the disinfection of clothes and for bedding, by placing the clothes very loosely on the iron bars of a bedstead, and by setting a few of the lighted lamps on the floor beneath the bedstead, a foot or two away from the clothes themselves. In a very little time the whole of the clothes are saturated with the sulphurous acid vapour, if the room containing the clothes be closed.

The object of adding the benzoline is to give steadiness to the liquid during the process of burning. A common benzoline lamp is quite sufficient for the purpose, and if the flame from it throws off also some free carbon, as smoke, it does good rather than harm, except in respect to dust which is easily brushed away.

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#### *INTESTINAL OBSTRUCTION FROM CHARCOAL.*

 PATIENT who was lately under my care, suffering from chronic obstruction in the intestinal canal, probably in the descending colon, took, on his own account, freely of charcoal powder, often a drachm a day, to relieve flatulency. The result was an all but fatal obstruction. Fortunately under the use of enemata coupled with persistent but gentle abdominal friction, relief was obtained, although stercoraceous vomiting once occurred. The excretions causing the obstruction were coated with carbon, and portions of carbon also passed in the free state. This is the second case in my practice in which charcoal powder has caused intestinal obstruction.

## *SCHOOLS OF PHYSIC; PAST AND PRESENT.*

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**M**EDICAL Schools in London as distinct institutions date from the time of William and Mary. Previously to that time, men intending to enter physic would go to London to "walk" the hospitals for a few months, but nothing more. If they wished to learn anatomy they would go to Paris, made famous as a school of anatomy by Sylvius, or, as Harvey did, they would go to some Italian school, rendered immortal by the teachings of Mondinus, Vesalius, Fallopius, Fabricius, a long time the anatomists of the world. In England the study of anatomy was severely discountenanced, and for many centuries no one had the courage to follow it as a branch of education. At the College of Physicians a learned member might expound to his fellows the parts of the body; at Barber-Surgeons' Hall a "Reader in Anatomy" might have a class; but schools for the uninitiated there were none. In the reign of William and Mary, there came over to England, from Paris, a Frenchman named Bussière: Bussière was a refugee, and it is to our national credit to say that even in those times this fact made no difference in the reception accorded to him. But Bussière was a stranger, and had nothing to exist on save his wits. What should he do? In his



own country he had studied anatomy, and had for a period taught anatomy as a sub-lecturer, or demonstrator. He thought that he saw an opening for an anatomical school in London, and, with great courage, for no movement could have been more unpopular, he commenced to teach the structure of the body, in a scientific manner, to all, professed or as yet unprofessed, who chose to enter his prelections. Bussière fought out his difficulties boldly, and, in spite of prejudice, laid the foundation of scientific medical teaching, based on anatomy, in the metropolis of England. He did a great work, which has scarcely ever been recognized, and he did it well.

After Bussière, who probably returned to France, anatomy found an exponent in a man of science by the name of Cooper, who was famous as a teacher, and who took pupils into residence with him. In 1703 Cooper received into his house a student from Leicester by the name of Cheselden, and Cheselden having soon acquired a perfect knowledge of anatomy, according to the estimate of his contemporaries, became in 1710-11 a teacher himself, and continued as such until 1729, when, after a career as a professor which had previously no parallel, he resigned the duties of the office, that he might retain, without over-taxation, the labours of his then extensive practice as the leading surgeon of his time. Five anatomists at least, Douglas, Nicholls, Fowler, Sharp and Nourse, continued to teach the science until the time of William Hunter, who, in 1746, founded his famous school, transferred in 1780 to Windmill Street, Haymarket. From this school of William Hunter, the teaching of medical science, with anatomy always at the root of it, has been continued as a distinct art ever since; giving to science, as teachers, in it or in rival

schools, the well-known names of John Hunter, Cruickshank, Baillie, James Wilson, Charles Bell, Herbert Mayo, John Shaw, Brodie, Brookes, Dermott, Carpue, the brothers Grainger, Pilcher, Cæsar Hawkins, who was lately with us, and last, but not least, Samuel Lane, who is still with us.

From the way in which medical schools originated in London, from the absence of a University in the Metropolis, they were at their commencement private speculations. The schools were quite distinct from hospitals, and the student who came to pass through his terms entered at one of them for anatomy, somewhere else for physic, and so on ; each teacher in any given department rivalling with some other teacher, without any kind of bondage, and in the most honourable way. The result was that the best teacher got the best classes, and by receiving a sufficient income from teaching to make the work worth having, he often devoted his whole life to his art, and did his duty unexceptionally. Old men now, who remember Brookes, speak with pride of the anatomical teaching which he gave, and still say to the youngsters that aspire to fill their place, " Ah, if there were but another Brookes. He was the man ; he did strange tricks ; kept an eagle or two ; filled his breakfast-room so full of anatomy that you could not sit down without removing a specimen from your chair ; went occasionally into a tantrum, but, taught anatomy ! " And, as a result, the pupils came. He was wont indeed to compare himself to that Roman centurion who said of his soldiers, " They always come when I call them, but sometimes they come when I don't call them."

Brookes' anatomical school was at the bottom of the street leading from Blenheim Steps, Oxford Street, on

the south side, not far from Oxford Circus, citywards. It was called the Anatomical Theatre and Museum. It was a large school fitted up with every convenience. When I came to London in 1849 it was still in good preservation, but was transformed into a dispensary, "The Blenheim Street Free Dispensary." The Physicians' room was that which Brookes had used as a study: the Surgeons' room had been his private dissecting room. The patients' waiting-room, a large and spacious hall, had been the Museum. Below and to one side of the Museum was a domed structure of glass covering a room in which some metal workers hammered away all day long: this was the old anatomical theatre or lecture hall, with a little yard by the side of it, where the eagles, said to be of "carrion kind," were once kept and fed. The school was handsomely decorated, and so substantially that the decorations remained in capital trim all through the Dispensary period. The decorations were of the Egyptian, or Syro-Egyptian style, a style which in the latter part of the last and early part of the present century was very popular. John Hunter adopted it at his country house at Earl's Court, and Mrs. Hunter's boudoir, still remaining, bears traces of it even now. The roof of Hunter's drawing-room retained a fine specimen of this decoration until a few years ago.

Brookes' school, as a building, is not dead yet. It was a dyer's establishment the last time I saw it, not many months ago. The entrance door can still be made out, and the places high up on each side the door on which two statues once stood, Harvey on the right, Vesalius on the left side, are detectable.

For some time, cotemporary with Brookes, and for a considerable time succeeding him, there was another

medical school in Gerrard Street, Soho ; called, from its locality, " The Gerrard Street School " ; and, from the name of its principal teacher, " Dermott's School of Anatomy and Medicine." Dermott, who had been a pupil of Brookes, was an anatomist of a very high order. He was the author of some admirable anatomical plates, and was a skilled expositor. I have known more than one of his pupils, and from them I gather that he was almost as popular as his master. His school, after he had ceased to teach and demonstrate, became a dispensary, and remains so still. Dermott had associated with him one or two other excellent teachers in different departments of medical service. Amongst these was Dr. Ryan, who taught Medicine and Obstetrics, and published in the *Medical Gazette* some useful lectures.

There was also a private school in Aldersgate Street, called the " Aldersgate Street School of Medicine," a very famous place in its day. Dr. Robert Willis lectured there, and before him, for a short time, his master, John Abernethy. This school ceased to exist about 1854. It possibly was one of the oldest, dating from Nourse's School of Anatomy, 1730-35, though I cannot trace it down.

Dr. Carpue, the well-known anatomist, whom Tom Hood immortalized in his " Mary's Ghost,"—

" I can't tell where my head is gone,  
But Dr. Carpue can ;  
As for my trunk, it's all packed up,  
To go by Pickford's van,"—

held his lectures in Dean Street, Soho, in a house, part of which was afterwards used by a jeweller named Welsh. A member of the Welsh family was once under my care, and before the place was altered, I was

shown Carpue's theatre at the back of it. It was a very convenient structure for its purpose.

The last of the free or independent schools of medicine was Lane's, called, for many years, the St. George's School of Medicine, and later on the Grosvenor Place. No trace of this school now remains. It stood round by the side of St. George's Hospital, Hyde Park Corner, immediately out of Grosvenor Place. Originally Grosvenor Place was continuous from the hospital, with nothing but a courtyard separating it from the houses. The yard was the famous Tattersall's, where horses were bought and sold as they are now at Tattersall's, Knightsbridge. On one side of the entrance to this yard was the hospital, on the other side a corner house, Mr. Lane's, No. 1, Grosvenor Place. The school was at the back of Mr. Lane's house, with a short passage leading to it from the house. The site of the school is at present a grass plot, with trees filling it.

When St. Mary's Hospital was completed, Mr. Lane, who became one of the surgeons to St. Mary's, gave up his old school to Dr. Edwin Lankester, who conducted it for a short time, and then made it over to the Faculty of Lecturers, which he had organized. I being, at the time, one of the lecturers, became Dean of the school, which office I held, with that of lecturer of physiology, until the lease of the building fell in to the ground landlord, the Duke of Westminster. The Duke demolished the building, and therewith finished off the work which Bussière, the Frenchman, commenced under William and Mary. It is a curious coincidence that the last French anatomist in London, and lecturer on anatomy at the last anatomical school, was, like Bussière, an exiled Parisian professor. This



was our distinguished anatomical colleague at the Grosvenor Place during its latter days, the late Dr. Amadée Deville.

Many a good student's story has been told in and respecting the last of the private schools, its well-known dissecting room porter, James Crow, styled by the students "Jim Crow," and his man "Friday." But, student as I still am, I cannot tarry to wake up those dead echoes.

I must, nevertheless, wait one moment to pay a word of dear and deep respect to the last founder of the last of the great private schools of anatomy in London, Mr. Samuel Lane. As an anatomical teacher, Lane was one of the most distinguished. His command over his students was as perfect as was his knowledge of his subject. He had the most easy and rapid method of teaching ; and he compiled a museum, which, though not large, was as ready for work as any that can well be conceived. Equally admired as a teacher, and respected as a man and a gentleman, Mr. Samuel Lane closes the line of independent founders of London medical schools with a dignity and character which any of his illustrious predecessors, hardly excluding William Hunter himself, might be proud to have owned, and throughout life displayed.

The private schools, as will have been surmised, were scattered about to meet the localities of the different hospitals. Brookes' school commanded the Middlesex ; The Aldersgate, the City Hospitals ; Dermott's, the Westminster ; Lane's, St. George's.

Gradually a change came over the London teaching. The members of the hospital staffs, beginning as they did to find out the gains of teaching, began to coquet with the schools, to ask to be allowed to lecture at

such and such a school, and, by-and-bye, to make themselves a part of the school. There were a good many officers in the hospitals, and as no two men could lecture on the same subject, at the same time, it became necessary to make chairs to accommodate the crush of anxious teachers; so, at last, around the anatomy chair there grew up chairs for every possible department: a chair for chemistry, a chair for surgery, a chair for medicine, a chair for *materia medica*, and many more. The rage was for chairs.

Then a new feature presented itself. The larger hospitals, like Bartholomew's, Guy's, St. Thomas', and the London, established their own schools; and next the hospital staffs generally began to ask, Why should we put ourselves under any obligations to private teachers? Why should we not open, on our own account, at our hospital, an anatomical theatre and school? A lucky idea, and one which soon found favour. How the governors of hospitals were first inveigled into the movement I cannot say, but certain it is that, the example once set, every hospital, of any size, in the metropolis, felt a call to teach, and so far succeeded, that now every considerable hospital has its school with its chairs. With this movement there was developed another, which, in an all but imperceptible manner, has grown into a strong and firmly organized system of medical policy.

About the beginning of the present century some of the corporations in physic and surgery began to see that they could increase their influence and wealth by extending the work of examining for diplomas, according to their privileges; and by a coincidence, in no way unnatural, the representatives of the examining boards discovered that it was not, by any means, disadvan-

tageous for them, to insist upon a mild attendance of students on certain lectures. In this way sprang up what is called the curriculum system, a system which compels a student, not merely to pass a certain examination, but, previously, to attend, *nolens volens*, courses of lectures, and courses of practice delivered by certain gentlemen, who are known as "recognized teachers," and who may or may not be examiners also.

With the decline of the private schools, the whole of the teaching fell practically into the hands of the hospital physicians and surgeons. They, too, with exceptions in the Apothecaries Company, were the rulers and the examiners in the corporations which granted the diplomas, by natural virtue of their eminent positions as officers at the hospitals ; and when schools grew up within the hospitals they, by a similar natural kinship, become teachers. They did not, however, become teachers in their respective chairs as their predecessors of the private schools did, namely, by slow and practical degrees of art, but by the accident, chiefly, of their official connection with the hospital.

Whether the system founded on this basis is sound has not yet been proved. It has its conveniences and it has its disadvantages. Unbiassed entirely, and altogether free from any interest one way or the other, I may, perhaps, with some usefulness to the public generally, whose servant I am, indicate how the conveniences and the disadvantages are supposed to stand in relation to the common welfare. The conveniences of the current system are, usually, summed up in the following reasonings.

It is advantageous to any hospital to have a school of medicine attached to it. The school gives a certain *éclat* to the institution. It affords the members of the

staff of the hospital an opportunity of getting seats as teachers, without subjecting them to any artificial and excessive tests that they are specially fitted, or are gifted with powers and faculties for teaching of the highest quality. Thus it brings out plenty of plain practical teachers who know their work, who are provided with ample facilities for work, and who by becoming teachers are led, though teaching, to be better practitioners than they would be if they had no responsibilities of a professorial kind.

It is advantageous to a large hospital to have connected with it a body of young men readily and regularly trained to become its subordinate officers, its dressers, assistants, house physicians and house surgeons. These, in their respective posts, know the ways of the place and its requirements; they give good service to it, and, in return, obtain good practical instruction, destined to be useful to the people who will, ultimately, depend upon them for medical and surgical aid.

It is advantageous to the student to have all his class-work close at hand; to have everything brought, as it were, to him, so that he need not wander here and there, as the student of old time did, to find the teacher that suited him best, however far away from the scene of practical labour.

These are unquestionably great conveniences, and they who are most opposed to the present system, though they may writhe under it, ought to give to the conveniences the fullest and fairest consideration.

The opponents of the system on their side charge it with possessing disadvantages of the most disastrous character to teachers, to students, to the profession of medicine generally, and to the public at large. Their objections run somewhat in the following order.

They insist that the mode of election of teachers is not, as a rule, calculated to secure the best teacher in any department. A teacher obtained from within the walls of the hospital may prove to be gifted for his office ; he is, however, as likely not to turn out gifted. But all teachers should be gifted, and if the selection were proper all would be. As it is, a man with the most startling gifts can find no place unless he happens to drop into a favoured circle in which he will soon be absorbed and lost. In illustration, a man like the admirable private teacher of anatomy, Mr. Hind, may have every mental gift for teaching anatomy, may be an accomplished expositor, an enthusiastic leader in his art, a winner of the confidence and admiration of all who have felt the choice benefit of his instruction, an extra academical professor to the academical ; and yet, merely because his course of lectures is not delivered within the sacred walls of the hospital school, his certificates of attendance may not qualify his students for examination. He may do the work which the recognized lecturer ought to do, but the recognized must sign the qualifying document. The effect of this, it is suggested, is demoralizing to the young, since it handicaps, unfairly, honest, industrious, conspicuous talent, makes the privilege of licensing certain teachers a delusion and an imposture, and destroys, as it is thought, that enthusiasm for great teaching which is the very soul of professional ardour and ambition. Students, we are told, are made students for ever, when, gathering their behaviour from the great, they grow great by example. The illustrious teacher is to them the all in all. It is life to them, while they are students, to witness his method, to receive his mind. It is long life to them, afterwards, to say, I was the pupil of such



a professor,—a Liebig, an Owen, a Faraday, a Bunsen. On the other hand, for the student to receive no mind, to be launched into life touched by no fire of ambition for noble work and noble life, is death itself present and to come. Little wonder, say these critics, that students of medicine should be great in frolic. On what can they expend their exuberant energy if they have no heroes in their own sphere to applaud, to imitate, and to follow?

The argument is made to extend to the injurious effect of the system upon the teacher as well as the taught. The teacher must have no ambition. He must work in common bonds. Has he desire to stand forth and ability to feed desire, he must needs starve both. Has he genius, he must let it die. William Harvey, William Hunter, Vesalius, themselves, could be nothing but commonplace in such yokedom.

In further proof of this part of the argument, the remarkable circumstance is adduced that at the present moment there is not a teacher in the whole of medicine who can be justly placed in historical comparison with the greatest teachers of the past, either by work in the lecture room, the museum, the hospital, or the study. Some few who, springing from medicine, have left her, practically, such as Darwin, Livingstone, Graham, have made names to live, but how few within the pale. Medicine proper is falling headlong out of history. Of obituaries she leaves a harvest; of biographies, mere gleanings scarcely worth the collection. So runs the criticism.

Lastly, in respect to the direct personal conveniences of the present system in its application to the students of medicine, the opponents of the system adduce, on their side, that as students are ready at any moment to

steal away from their own schools to listen to any good outside teacher, who cannot shorten their studies by giving them a certificate of attendance, but who can shorten their labour by giving them the information required, it is absurd to set up the argument of saving time by connecting the school with the hospital. Many also argue that it is a narrow and mischievous system to maintain separate medical school colonies where individual peculiarities rather than general scholastic tastes are cultivated. While not a few are strong in the assumption that the crowding of a multitude of students into one hospital, because of the school attached to it, and the proximity of the anatomical to the clinical part of the teaching, are less conducive than may be desirable to the welfare of the sick within the hospital.

I have thus placed in their fullest forms the extreme opposing views on the medical scholastic system at this moment current in our British community. Those who represent the present system by approval of it, may be called the practicals, their opponents the enthusiasts.

Which has the best of the argument ?

The question is not very easily answered. The present system is certainly very unsatisfactory to many, and even to many who are actively engaged in teaching upon it. Why there is not a wider and freer expression on the subject is due, not to carelessness, not to absence of anxiety as to the future, but to the difficulty of determining what shall be done to make things better, and to give to true genius for the professorial art its complete freedom and strength in the medical sphere. It is acknowledged that in London, at least, there are too many schools ; that great teachers cannot be found for all the departments and chairs of so many, big and little, institutions. It is acknowledged that the pecuniary

inducements for great teaching can never sustain the highest educational work while the chairs are so numerous, the qualifications for them so easy, and while the required devotion to teaching, as a splendid but laborious art, is interrupted by other and responsible professional duties and labours. It is thought, therefore, very openly, that the time has come when a great change is required, a change which seems to be settling into three suggestions.

One of these suggestions is that there should be an amalgamation of schools ending in the formation of a grand faculty of medicine which, under the direction of a representative from all the schools, should form a few great centres or academies for the theoretical classes, leaving the hospital and clinical teaching, and that alone, to the hospitals, and to their medical and surgical staffs. If this were done, the best teachers from the hospitals would be drafted into the schools, where they would earn magnificent incomes free from all the anxieties and labours of medical practice, and where they would bring professorial work to the highest development.

A second suggestion is that the present medical schools connected with hospitals should be left as they now are, but that there should be no exclusion of free teaching. Let any man who knows he can teach, teach to his heart's content, and let his certificate rank with every other for examination. This would be perfectly just, would encourage friendly rivalry, and would permit any great teacher and organizer to compete, even with a William Hunter, for name and wealth and fame.

A third suggestion is to stop all theoretical teaching in connection with the public hospitals, and to leave such teaching entirely to individual enterprise. Under

such circumstances it is assumed that the private effort left perfectly free would meet every requirement, and that medicine would soon regain what she has lost, in teachers as renowned as were they who produced her grandest historical characters and individualities ; teachers who would attract from all the world the best students to become the best professors and practitioners when their turn should arrive.

Not because the wish is father to the thought, but because I honestly believe, from historical study, what the future of the matter will needs be, I, for my part, incline to the last-named of these suggestions. The first of them would leave medical education too much in the hands of central and interested electors. The second, though it looks uncommonly fair at first sight, would be unfair to the private teacher, however skilled he might be. His students in the hospital ward, as we found when this very plan was in force, would not get the same advantages as those who were in schools. And this alone, for students are extremely sensitive, would be fatal to the enterprise.

The third plan would meet every requirement ; it has been tried, and found efficient, producing the most magnificent,—indeed, the most historical,—names and results ; and this alone is sufficient to commend it, if its coming were a mere matter of choice. There is, however, in its favour more than choice ; there is necessity. The hospital system altogether is failing. The people see that as, according to the theory of our constitution, no person ought to die from poverty, and no person ought to be allowed to suffer from disease without receiving medical relief paid for by the community, so every sick person who is too poor to be treated at home ought to be treated in the people's infirmary, and to have there,

apart from all capricious voluntary help afforded through outside institutions, the best medical and surgical assistance that can be provided. With this knowledge even the wealthy are declining to support the voluntary hospital, and when the hospital fails its school must fail, if it do not fail before from popular disfavour against itself.

It seems to me, therefore, inevitable that we shall come back to the old and splendid system of medical teaching of the past; that once again every medical practitioner will have his pupil, who for three years or so will acquire an amount of sound practical knowledge and skill, which nine out of ten students miss in their hospital career; that the pupil thus prepared—by a master who has himself benefited by having been a master—will pass into the hands of greater masters, who make the art of teaching the sole art of their lives; and that entering the practice of his profession by an examination, separated entirely from all other interests except those of the public expressed through the State, the qualified practitioner will begin his career with an enthusiastic as well as an untrammelled mind.



## COTEMPORARY PRACTICE AND LITERATURE.

*"Every physician will and ought to make observations from his own experience ;  
but he will be able to make a better judgment and juster observations by  
comparing what he reads and what he sees together."*—FRIEND.

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### JOSEPH JONES ON ALCOHOL AS A POISON.

**I**N the last ASCLEPIAD there was noticed at some length the report on Small-pox recently published by Dr. Joseph Jones, President of the Board of Health for the State of Louisiana. This quarter there comes to hand his introduction to the Annual Report of the Board of Health to the General Assembly of the State of Louisiana, 1883-4.

This introduction, like all the work of its author, is crowded with the most valuable and interesting facts and principles. The chapter on the preventible causes of disease is so rich, it is indeed difficult to make a selection by way of illustration. As tending to elucidate a subject which at the present moment is attracting an unusual share of attention in this country, rather than from any peculiar merit above the other parts of the work, I select a portion of the chapter on the poisonous effects of alcohol.

“It can be clearly demonstrated by facts,” says Dr. Jones, “that alcohol is the most destructive of all poisons to the human race, and to this general assertion the citizens of New Orleans form no exception. Thus in 1857, in the city of New Orleans, the deaths from intemperance and delirium tremens numbered 84; 1858, 139; 1859, 138; 1860, 175; and during the four years specified 526 individuals perished in New Orleans from the direct effects of alcohol.

“A more direct demonstration of the effects of alcohol in producing disease and death can be derived by a careful examination and consolidation of the statistics of the Charity Hospital, which must be regarded as the great pathological thermometer or weather-gauge of the health of New Orleans.

“The records of the Charity Hospital of New Orleans show the following important facts:

“During a period of thirty-four years—1842-1862, 1864-1880—there were treated in the wards of this institution 4,694 cases of delirium tremens and mania a potu, of which number 914 (19·4 per cent.) died; during the same period 1,873 cases of intemperance were treated, of which 97 (5·1 per cent.) terminated fatally.

“That is, during a period of thirty-four years, in the city of New Orleans, an army of 6,567 trembling, maddened, raving, drunken men and women, poisoned by alcohol, were conveyed to the wards of the Charity Hospital, and of which number more than one-seventh, or 1,011 (one thousand and eleven), perished.

“To estimate fully the destructive effects of alcohol, we must add the cases of death from the following diseases, treated in the Charity Hospital during the same period of thirty-four years, 1842-1880:

| DISEASES.                | CASES. | DEATHS. | PER CENT. |
|--------------------------|--------|---------|-----------|
| Cirrhosis of Liver . . . | 390    | 240     | 61·5      |
| Abscess of Liver . . .   | 181    | 88      | 67·1      |
| Ascites . . . . .        | 981    | 428     | 43·6      |
| Gastritis . . . . .      | 1,673  | 124     | 7·4       |
| Anasarca . . . . .       | 896    | 269     | 30·0      |
| Hepatitis . . . . .      | 806    | 221     | 27·4      |

“ From the diseases just specified, which, as is well known, are in a large measure dependent upon the use of distilled liquors in hot climates, there resulted 4,877 cases and 1,370 deaths.

“ It would be fair to estimate that, during the last thirty-four years (allowing for the relative proportion of deaths and cases of sickness in the city and Charity Hospital), alcohol has directly caused at least 20,000 cases of sickness and occasioned over 5,000 deaths in the city of New Orleans.

“ It is impossible to form any correct estimate of the pecuniary loss to the city and State by the destructive effects of alcohol, for of all poisons it occasions the greatest physical, mental, moral, and industrial loss.

“ It is the province of our wise, learned, and incorruptible legislators to devise ways and means to regulate the unrestrained sale of distilled liquors, and to command the peace at all times and in all places. It is not our intention to inquire whether expostulation would be useless and entreaty despised, and legal proceedings utterly unavailing, through the intricacies of law processes and the failures of the officers of justice; but, as President of the Board of Health of the State of Louisiana, I desire by stern facts to arouse the public to a sense of the greatest and most destructive of all poisons.”

## VACHER ON THE INCUBATION OF INFECTIOUS DISEASES.

**I**N my paper on the incubation of infectious diseases, read before the Association of Medical Officers of Health, and afterwards published in the *Medical Press*, and in the volume entitled the "Field of Disease," I divided diseases marked by periods of incubation into five sections, headed, *shortest, short, medium, long, longest*. In the series of shortest period of incubation, from one to four days, the diseases included are, *malignant cholera, malignant pustule, plague, catarrh, and dissection-wound fever*.

In the series of *short* incubation, from two to six days, the diseases included are, *scarlet fever, diphtheria, dengue, erysipelas, yellow fever, pyæmia, influenza, pertussis, glanders, farcy, grease, croup, puerperal fever*.

In the series of *medium* incubation, five to eight days, the diseases included are, *relapsing fever, gonorrhœa, vaccinia, inoculated small-pox*.

In the series of *long* incubation, ten to fifteen days, the diseases included are, *natural small-pox, varicella, measles, rotheln or German measles, typhus, typhoid, mumps, malarial fever*.

In the series of *longest* incubation, forty days or more, the diseases included are, *syphilis and hydrophobia*.

In a paper on "*The duration of the latent period, the fever, and the infectiveness of the Exanthemata and some of the allied diseases*," Mr. Francis Vacher, the Medical Officer of Health for Birkenhead, has recently put on record his calculations on the same subject, in so far as they relate to eleven of the diseases I

have quoted, namely *scarlatina*, *diphtheria*, *idiopathic erysipelas*, *modified small-pox*, *small-pox*, *varicella*, *measles*, *German measles*, *typhus fever*, *typhoid fever*, *mumps*.

In some particulars Mr. Vacher's calculations accord with my own, but he divides the stages differently. He calculates the TIME:—(a) *From inception of the disease to beginning of the eruption.* (b) *From first precursory symptoms to the beginning of the eruption.* (c) *From the beginning of the eruption to the cessation of the pyrexia.* (d) *From the beginning of the eruption till the patient ceases to be infective.*

Comparing Mr. Vacher's table with my own, I find the following resemblances and differences. To scarlet fever he gives "a few hours to four days' incubation"; I give two to six days, with instances of hours only. To diphtheria he gives five days; I give two to six. To erysipelas he gives a mean of five days; I give two to six. To small-pox he gives thirteen days; I give ten to fifteen. To varicella he gives thirteen days; I give ten to fifteen. To measles he gives fourteen days; I give ten to fifteen. To German measles he gives fourteen days; I give ten to fifteen. To typhus he gives nineteen days; I give ten to fifteen. To typhoid he gives twenty-one days; I give one to fifteen. To mumps he gives eighteen days; I give fifteen. In relation to the last three named diseases there seems a considerable difference, but it should be remarked that Mr. Vacher's figures, as they have been rendered above, are, in each case, a mean derived from a range of period extending, in different cases, over many days. For instance, in typhoid his mean of twenty-one days is calculated from a range of one day to twenty-eight.

It is to be hoped that all medical readers will make



themselves familiar with Mr. Vacher's valuable table, and with his essay altogether. It refers to a subject on which we are questioned every day, on which we are very loose in our estimates, and on which, if Mr. Vacher be right, we are never very likely to be exact. If it be true, for example, that typhoid has a range of incubation of twenty-eight days, with what certainty can we speak on that disease? I expect, however, that there is in nature no such aberration, and that the discrepancy is due to some errors of our own in observation. Either we get imperfect evidence as to facts, or we date from uncertain starting-points, which is of all things the most common cause of error.

The last part of Mr. Vacher's table, referring as it does to the period when a person who has suffered from infectious disease ceases to be infective, is most instructive, and, approximately, very sound. He designates it as the "*time from the beginning of eruption till the patient ceases to be infective*," and he records the time as follows in days:—*Small-pox*, 56 days; *modified small-pox*, 35; *chicken-pox*, 17; *measles*, 27; *German measles*, 14; *scarlatina*, 49; *diphtheria*, 28; *idiopathic erysipelas*, 35; *typhus*, 21; *typhoid*, 28; *mumps*, 21. These, as far as I remember, are the first systematic calculations on the point advanced.



BRUDENELL CARTER ON CATARACT.



R. BRUDENELL CARTER, able and accomplished author on all subjects he touches, is never happier than when treating on matters which, professionally, he has specially studied. A new work of his on "The Modern Operations for Cataract" has just been published by Messrs.

Macmillan, and will ensure the closest attention of the surgical world at home and abroad. The work consists of three Lettsomian lectures, delivered before the Medical Society of London, and published at the request of the Society. The first lecture deals with the methods of operation, with the progress which has been made, and with the general results. The second lecture treats on the maturity of cataract, on the conditions which justify early operation, and on methods of procedure. The third lecture dwells on the accidents incidental to early operation, the complications which may arise after the operations, and the after treatment. In this chapter the advantages and the disadvantages of using anæsthetics during operation are discussed. Mr. Carter thinks that for general use in ophthalmic surgery the anæsthetic mixture called the A.C.E.—one part by measure of absolute alcohol, two of chloroform, and three of ether,—is the best. Chloroform pure and simple he would banish altogether.

The Lettsomian lectures of the Medical Society of London have very often been excellent both in science and practice. These of Mr. Carter maintain, eminently and distinctively, both qualities.



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